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Volume L SEPTEMBER 1960 Number 4

ARTICLES

The First Two Decades of the American Economic Association — *A. W. Coats* 555
Operations Research — *Robert Dorfman* 575
Patterns of Industrial Growth — *H. B. Chenery* 624
Credit Controls and Financial Intermediaries — *D. A. Alhadeff* 655
The Radcliffe Report and Evidence (Review Article) — *J. G. Gurley* 672

COMMUNICATIONS

The Public Debt: A Burden on Future Generations? — *W. G. Bowen, R. G. Davis, D. H. Kopf* 701
Capital Formation in Underdeveloped Countries — *Nathan Rosenberg* 706
Wages and Interest—A Modern Dissection of Marxian Economic Models:
Comment — *F. M. Gottheil* 715
Reply — *P. A. Samuelson* 719
The Pure Theory of International Trade:
Comment — *H. G. Johnson* 721
Inflation: Correction and Restatement — *F. D. Holzman* 723

BOOK REVIEWS

ASHTON, Economic Fluctuations in England, 1700-1800, by R. E. Cameron 748
BELL AND SPAHR, editors, A Proper Monetary and Banking System for the United States, by N. H. Leonard, Jr. 771
BOTHA, A Study in the Theory of Monetary Equilibrium, by N. V. Breckner 770
BOWEN, The Wage-Price Issue—A Theoretical Analysis, by P. L. Kleinsorge 735
BRAHMANANDA, Studies in the Economics of Welfare Maximization, by W. B. Palmer 732
BRITTAIN, The British Budgetary System, by T. R. Snavely 773
CAVES, Trade and Economic Structure: Models and Methods, by M. O. Clement 774
CAVIN, J. P., editor, Economics for Agriculture: Selected Writings of John D. Black, by E. O. Heady 807

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DAY AND BEZA, Money and Income, by J. Cohen 737
DE CHAZEAU AND KAHN, Integration and Competition in the Petroleum Industry, by J. S. Bain 799
DUPRIEZ, Philosophie des conjonctures économiques, by J. Amuzegar 740
EDWARDS, The Price Discrimination Law, by M. A. Adelman 790
FRIEDMAN, A Program for Monetary Stability, by L. S. Ritter 765
GAITSKELL, Gezira: A Study of Development in the Sudan, by R. J. Irvine 751
GOLDSMITH AND SAUNDERS, editors, The Measurement of National Wealth. Income and Wealth Series 8, by R. W. Pfouts 760
GRAMPP, The Manchester School of Economics, by G. W. Hilton 742
GUTENBERG, Untersuchungen über die Investitionsentscheidungen industrieller Unternehmen, by R. W. Bergner 788
HAILSTONES, Basic Economics, by A. D. Butler 726
HARRINGTON AND JACOBS, editors, Labor in a Free Society, by J. V. Spielmans 812
HERFINDAHL, Copper Costs and Prices: 1870-1957, by E. S. May 810
HOLBIK, Italy in International Cooperation, by H. S. Miller 753
HOUSSIAUX, Le pouvoir de monopole. Essai sur les structures industrielles du capitalisme contemporain, by E. Grunberg 802
HUGHES AND LUARD, The Economic Development of Communist China 1949-1958, by C. M. Li 746
ISARD, SCHOOLER AND VIETORISZ, Industrial Complex Analysis and Regional Development: A Case Study of Refinery-Petrochemical-Synthetic Fiber Complexes and Puerto Rico, by T. Marschak 756
KANTOROVICH, Ekonomicheskii raschet nailuchshego ispol'zovaniia resursov (Economic Calculation of the Optimum Use of Resources), by R. W. Campbell 729
KEIRSTEAD, Capital, Interest and Profits, by J. C. Hubbard 731
KENT, Corporate Financial Management, by R. H. Wessel 789
KLOTEN, Die Eisenbahntarife im Güterverkehr, by K. W. Roskamp 805
KORNAI, Overcentralization in Economic Administration—A Critical Analysis Based on Experience in Hungarian Light Industry, by N. Spulber 763
KRIEGHOFF, Technischer Fortschritt und Produktivitätssteigerung, by H. Brems ... 745
LADOR-LEDERER, Capitalismo mondiale e cartelli tedeschi tra le due guerre, by O. Ornati 781
MARCHELL AND LECAILLON, La répartition du revenu national. Vol. 3, Modèles classiques et marxistes, by J. M. Ferguson 734
MASON, editor, The Corporation in Modern Society, by C. C. Bosland 795
MASSÉ, Les choix des investissements, by K. J. Arrow 783
MATTHEWS, The Business Cycle, by D. Hamberg 738
NYKRYN, Obchodni operace v československem zahraničnim obchodu, by K. Holbik.. 776
PROCHNOW, editor, The Federal Reserve System, by W. Wright 768
ROSTOW, Planning for Freedom—The Public Law of American Capitalism, by G. P. Adams, Jr. 761
SOLOMON, editor, The Management of Corporate Capital, by L. E. Dobriansky 785
TEW AND HENDERSON, editors, Studies in Company Finance: A Symposium on the Economic Analysis and Interpretation of British Company Accounts, by D. A. Snell 786
WAFFENSCHMIDT AND OTHERS, Deutsche Volkswirtschaftliche Gesamtrechnung und ihre Lenkungsmodelle 1949-1955, by E. M. Fels 749
WARNER AND FUCHS, Concepts and Cases in Economic Analysis, by W. J. Mead 728
WHITTAKER, Schools and Streams of Economic Thought, by H. W. Spiegel 744
YOUNG, Understanding Labor Problems, by H. W. Davey 814
ZOOK, editor, Economic Development and International Trade, by W. B. Kelly, Jr. ... 780
Czechoslovak Economic Papers, by M. V. Condoide 725
Économies des démocraties populaires—les régions économiques en U.R.S.S., by H. T. Reed 758
Radcliffe Report, by J. G. Gurley (a review article) 672
Toward a New Energy Pattern in Europe—Report of the Energy Advisory Commission, O.E.E.C., by F. G. Adams 808

OTHER DEPARTMENTS

Titles of New Books 816
Periodicals 834
Notes 850
Titles of Doctoral Dissertations 864

The American Economic Review
VOLUME L SEPTEMBER 1960 NUMBER FOUR

THE FIRST TWO DECADES OF THE AMERICAN ECONOMIC ASSOCIATION

By A. W. Coats*

As this month marks the 75th anniversary of the formation of the American Economic Association it is an appropriate moment to recall the state of American economics in 1885 and to reassess the changing character and fortunes of the organization in its formative years. Earlier accounts have emphasized the theoretical and policy issues that divided the so-called "old" and "new" schools of economics [15] [16] [17] [18] [9, Ch. 9] and have unduly neglected other, less obtrusive, aspects of the story. The following pages are designed to supplement the published record and to place the Association's early history in a wider intellectual and social context.

I. *The Inauguration*

The American Economic Association was officially inaugurated on September 9, 1885 in the Bethesda Parish building at Saratoga Springs, New York, following discussions among a miscellaneous group of scholars, ministers and social reformers who were attending the second meeting of the American Historical Association. The initiator of this venture, Richard T. Ely, a vigorous young member of The Johns Hopkins University faculty, was following the lead of his senior colleague in the history department of that institution, Herbert Baxter Adams, who was the founder and secretary of the older association. The dramatis personae and the location reveal the character of the enterprise,[1] for there was at that time no independent academic discipline of eco-

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[1] Apart from the religious element in the setting, Saratoga Springs was the regular meeting place of the American Social Science Association, under whose auspices the AEA was created [5, p. 548].

nomics with a recognized corps of practitioners; and religious inspiration and reformist zeal were to play a major role in the organization's early history. The personal link between Ely and Adams symbolizes the connection between the rising tide of economic thought and the contemporary movement towards genuine "university" education in America. Both men had embraced the Germanic conception of higher education and scholarship which formed the intellectual core of this movement; and they had received encouragement and advice from D. C. Gilman, the distinguished first president of Johns Hopkins, so that the two associations illustrate the exuberant activity of the institution at the head of the "academic procession."

Just as the process of adapting the German university to American needs and conditions required the cooperation of established scholars, administrators and trustees, so the process of launching a professional association of economists would have failed if Ely had relied exclusively on the support of the young men who had recently completed their studies in Germany. The idea of an economic association was undoubtedly German in origin; but an earlier attempt to form an association in 1883-84 had failed because it had been modeled too closely on the *Verein für Sozialpolitik* and because its sponsors had placed too much emphasis on the role of the state in economic affairs [17, pp. 133-35, 296-99]. The program of this abortive Society for the Study of National Economy had met with a chilly reception not only because its provisions entailed too specific and radical a break with the past but also, one suspects, because its progenitors, E. J. James and S. N. Patten, represented the University of Pennsylvania, an institution that was out of touch with the contemporary "university" movement and had long been regarded with suspicion as the home of protectionism and the center of opposition to orthodox political economy.[2]

Ely had formulated a plan for an association of ". . . economists who repudiate *laissez-faire* as a scientific doctrine . . ." as early as 1884, but he deferred his proposal until it was clear that the James-Patten scheme had failed [14, F. A. Walker to Ely, Apr 30 1884] [15, p. 55*n*]. When he drafted his platform for the American Economic Association in the following year, Ely toned down his references to the role of government in economic and social life; the names of James and Patten did not appear among the signatories of the "call" to Saratoga, although they were among the founder-members of the new body; and the prestige of Johns Hopkins probably added weight to his appeal.

[2] Some years later E. J. James recalled his efforts to form an association and remarked that some of the academic economists he consulted believed that no new body was needed since the ASSA, which had been founded in 1865, "practically performed the only available function of such an organization" [15, p. 109].

But whatever the reasons, Ely's call met with a warm response not only from the young scholars who had been impressed by the reigning German school of historical economics but also from leading historians, prominent university presidents and ex-presidents like Gilman, Andrew White, C. K. Adams, W. W. Folwell, and Francis A. Walker (who was also a distinguished representative of the older generation of economists), such outstanding liberal ministers as Lyman Abbott and Washington Gladden, and officers of the American Social Science Association.[3]

II. *Social Science versus Social Reform*

Notwithstanding its broad and influential support, the American Economic Association was fully established only after a struggle for recognition and support that continued intermittently throughout a seven-year probationary period. These initial difficulties cannot be explained merely by reference to the dispute between the old and new schools of economics over questions of scientific method, economic theory and public policy that were debated at length in *Science* magazine in 1885-86 and discussed in many other learned and popular journals and newspapers.[4] Certainly this dispute had an important bearing on subsequent doctrinal developments; but there was a deeper question of principle involved, namely: how far was the Christian impulse to social reform compatible with the scholarly impartiality deemed appropriate to a scientific body? The effort to reconcile these two aspects of the Association's original purpose came to be the constant preoccupation of its officers, and had a marked impact on its character and early development. Since both the public and the academic reputation of the organization were at stake, neither of which could safely be ignored by its members in view of their peculiar and sometimes uncomfortable involvement in contemporary social issues, this matter merits careful examination.

The difficulties involved in the task of reconciling scholarly impartiality with reformist zeal were largely responsible for the gradual attenuation of the original platform or statement of principles. Ely's initial draft was significantly ". . . modified in the direction of conservatism" [16, p. 144] at the inaugural meeting, before it was incorporated in the Association's constitution, and was published with the proviso that it was not to be regarded as binding upon individual mem-

[3] It has been suggested that the more conservative members of the ASSA joined the AEA [5, p. 520]. For a full account of the inaugural meeting see [18].

[4] The argument began with E. J. James' hostile review of Simon Newcomb's *Principles of Political Economy* in *Science*, 6 (1885) pp. 470-71, which brought replies and rejoinders *ibid.*, pp. 495-96, 517-18, 538, 563 and was formally continued in the ensuing two volumes with articles by most of the leading contemporary economists.

bers. Nevertheless it still contained references to the positive role of the church, the state and science in the solution of social problems by the ". . . development of legislative policy"; and these references provoked the hostility of those defenders of the received tradition by whom, as Francis A. Walker later recalled, *laissez faire* ". . . was not made the test of economic orthodoxy, merely. It was used to decide whether a man were an economist at all."[5]

To regard this hostility simply as an expression of opposition to the proposal to increase the functions of the state would, however, be a mistake, for there were some who doubted whether it was proper for a supposedly impartial scientific body to subscribe to any policy recommendations whatsoever. Ely himself had acknowledged the dangers of partisanship when he complained, in his original draft, that ". . . economists have been too ready to assert themselves as advocates," although he defended the platform somewhat inconsistently on the grounds that ". . . it is not easy to arouse interest in an association that professes nothing" [18, pp. 7, 19]. Simon Newcomb, an early opponent, may have been unduly caustic when he remarked that in Ely's view the Association was ". . . intended to be a sort of church, requiring for admission to its full communion a renunciation of ancient errors, and an adhesion to the supposed new creed" [31, p. 106]; but his comment reveals the attitude of one influential section of orthodox opinion.

Thus it soon became apparent to the Association's officers that the statement of principles might prove to be a liability, for there was ample evidence that it acted as a deterrent to some of the more conservative economists.[6] With the disappearance of the initial attitude of

[5] [40, p. 254]. It is significant that Walker was no young upstart seeking to overthrow the ruling doctrines, but a mature and highly respected innovator working within the received tradition. His difficulties were strikingly revealed in a letter to Ely dated April 30, 1884: "Perhaps no one has had more occasion than myself to feel the need of such moral support from fellow workers in political economy as might come from formal association and concerted action. When I first started out in 1874, I suffered an amount of supercilious patronage and toplofty criticism which was almost more than I could bear. Downright abuse would have been a luxury . . ." [15, p. 78].

[6] E. W. Bemis, reporting to Ely a conversation with J. L. Laughlin of Harvard, wrote: "He refuses to join the A.E.A. because it has any constitution save love of truth, for he does not know what he will believe five years hence and hence cannot belong to any class of disciples. Then too he took occasion to pour out his disapproval of an association which seemed desirous of excluding Prof. Sumner, if no one else, for though he claimed to dislike his evident dogmatism he preferred to be associated with men of opposite views and did not believe Prof. Sumner would make trouble.

. . . . . .

All this and much more he professed to say from a love of science and a dislike to divide its force" [14, Sep 29 1886].

Similarly, E. R. A. Seligman wrote to Ely: "I met Hadley [of Yale] at the Political Economy Club last Monday. When he heard that the 'confession of faith' had been dropped, he said he would join the Association gladly, and that he proposed to take a warm interest

exclusiveness,[7] which had been inspired by the fear that the organization might be captured by orthodox doctrinaires, there was a growing desire to enroll the Harvard, Princeton and Yale economists, who had hitherto held aloof more because of their suspicion of the platform than because of their uncompromising adherence to classical doctrine. By 1887 the proposal to modify or abandon the platform was under discussion, and John Bates Clark wrote to Henry Carter Adams [1, May 4 1887]:

> I had better tell you what I suggested to Dr. Ely as a compromise policy in the matter of [the] platform. I understand Pres't Walker wishes to drop the platform wholly. I had not anticipated quite so radical a move, though I have from the first thought that the platform would ultimately cease to be necessary. It does a certain work by giving character to the association during its earlier years. My proposal was that we agree not to oppose the abolition of the platform a year hence, provided the measure be not pushed this year. The friends of the measure will ask 'why not now if ever'? My idea is that the work of the platform is essentially temporary, and that two (practically three) years will prove long enough to retain it.

In fact events closely followed Clark's proposal, for the decision to drop the statement of principles was approved by the Association's council in December 1887, and ratified at the third annual meeting twelve months later [4, p. 86]. But although some prominent members insisted that no change of principle was involved, the vehemence of their denials suggests the importance they attached to this change of policy.[8]

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in us, and lend an active hand whenever possible. Put him down as a member" [3, Jan 2 1889].

One branch association, of which there were six by the end of 1888 [4, pp. 87-91], applied for membership without the statement of principles [14, Seligman to Ely, Jan 29 1888].

[7] In his autobiography Ely frankly disclosed this attitude. Although "we were anxious to win the great body of economists . . . [we] aimed to gather together like-minded men, congenial men who it was supposed could profitably work together. Not every economist was at first asked to join, although no economist who expressed a desire to join was refused enrollment" [17, pp. 141-42].

[8] Thus Seligman, the treasurer, wrote to Ely: "In answer to Dr. [Albert] Shaw's letter, I would say that in my opinion the change was not made in deference to any coterie—least of all the New York Nation coterie. . . . At the New York meeting President Walker stated that he was in favor of a change and had never favored the original platform because [it was] a platform. . . . None of the members present in the least desired any 'back down' but it was thought that the portions omitted were not essential, [and] might be misinterpreted . . . I do not think, nor did anyone suppose that the change could possibly be interpreted as denoting a change in the sentiment which dominates the Association. It was simply held that the welfare of the Society would best be promoted thereby . . ." [14, Jan 29 1888]. Shaw, of Minneapolis, a former student of Ely at Johns Hopkins, subsequently became editor of the *Review of Reviews*.

In this connection Ely's position is of particular interest, for despite the danger of overestimating the role of a single individual in the life of an organization, it is no exaggeration to say that in the early years of its history, the public response to the American Economic Association was largely determined by the various reactions to his work. As academic economists at that time, whether employed by state or private universities, repeatedly felt the pressure of public opinion [29, Part 2], this response could hardly be a matter of indifference to the rising economics profession. Apart from acting as the organization's initiator, first secretary (for seven years), promoter-in-chief and most ardent defender, Ely was a prolific writer whose studies of current labor, taxation and monopoly problems made him the most widely discussed economist in the country in the late 1880's and 1890's. As the most outspoken critic of the old school of American political economy, Ely drew the full fire of the diehard exponents of that doctrine; and even his closest friends among the younger generation of economists, J. B. Clark and H. C. Adams, warned that certain features of his work lent support to the repeated accusations that he was a socialist and a sentimentalist.[9] Their concern for Ely's security of tenure at Johns Hopkins was doubtless reinforced by their desire to protect the reputation of the American Economic Association and its adherents.

Of these two accusations, the former has been stressed by subsequent historians, and was undoubtedly a most serious matter at the time. Ely's boldly sympathetic study of the labor movement appeared shortly after the Haymarket bomb incident in Chicago had aroused widespread public alarm, and the story of Newcomb's damaging unsigned review in the *Nation,* the most influential journal of the day, which described Ely as a socialist and a man ". . . seriously out of place in a university chair," has often been told.[10] In the event, al-

[9] For instance, in reply to Ely's request for comments on his work, Clark, a very mild-mannered man, asked: "Does this passage justify the accusation brought against us of confusing the boundaries of economics and ethics? . . . is political economy ever hortatory?" and again: "Is the impression when the essay is in print heightened or diminished by the use of such strong expressions? Is not the rhetoric of restrained statement, if not of understatement, better?" H. C. Adams wrote: "The political economy of Mill does not 'glorify selfishness.' The expression is unscientific. A good deal of this part of your paper seems to me to be polemic rather than critical" [14, Vol. 18, Miscel. Scrap Books, Nov 1887].

[10] [9, p. 163]. Newcomb was then professor of mathematics at Johns Hopkins, and to the end of his life Ely believed that there was a local plot to expel him from the university [16, p. 146]. For contemporary references see [6, Ely to Clark, Dec 1 1886; H. C. Adams to Clark, Dec 16 1886; A. Johnston to Clark, Dec 17 1886; W. Gladden to Clark, Dec 25 1886].

In fact the current terminological confusion made it difficult to determine a writer's attitude to socialism. For example [7, pp. 566-67, 570, 577]. About this time H. C. Adams wrote in his personal diary: "I am a socialist—to tell the truth—with the very characteristic exception of accepting their plan of reconstruction" [1]. It is, therefore, hardly sur-

though this attack undoubtedly enhanced the suspicion of the Association in some quarters, it shocked economists of various shades of opinion, who rallied to Ely's support; and by persuading F. W. Taussig of Harvard to apply for membership, the first representative of the old school to do so, it directly strengthened the organization's representation among the "younger traditionalists." Indeed this proved to be but the first of many occasions on which the economists overcame personal and doctrinal differences in order to display their professional *esprit de corps* on behalf of one of their number who was threatened by outside interference.

In retrospect, however, the charge of "emotionalism" or "sentimentalism" resulting from Ely's religious fervor and his persistent stress on the inseparability of economics and ethics was more important in the long run; for it caused greater concern to his fellow economists and it contributed directly to his temporary break with the Association in 1892, a move which seriously threatened the unity of the organization. This matter has been unduly neglected by earlier commentators, although it sheds considerable light on the development of the economics profession and on the changing character of the Association, from that of an agency of social reform—a function that was dear to the hearts of the founder-members—to a more strictly scientific and scholarly body.

In order to explain the issues involved it is necessary to recall the state of American economic and social thought in the 1880's. At the centenary of the Declaration of Independence Charles Dunbar of Harvard had confessed that ". . . the United States have, thus far, done nothing towards developing the theory of political economy" which had become a body of rigid dogma both as expounded by its acknowledged leaders and by the numerous popularizers [12, p. 140]. Political economy had been assigned a subordinate role in the college curriculum, as a branch of mental and moral philosophy; its teachers, usually doctors of divinity, lacked interest and training in it, and the resulting doctrines comprised a blend of vulgarized Ricardianism and New England theology that offered little of relevance to contemporary life [32]. The intellectual (and financial) poverty of most American colleges had forced many able students to seek their advanced training abroad—in Germany rather than in England, not only because German academic life was flourishing, but also because religious influence was still strong in the best known English colleges. As J. Dorfman has shown [10],

prising that H. W. Farnam remarked, in a review of *The Labor Movement in America:* "Dr. Ely says—and he certainly ought to know—that he is no socialist. Yet much that he says sounds so much like what a good many of the socialists say, that he ought hardly to complain, if people occasionally mistake him for one" [20, p. 686].

German historical ideas had already met with a favorable reception in America in the 1870's, well before the flood of returning graduates had reached its peak. The flexibility of these ideas—in marked contrast to the received tradition—made them readily adaptable to domestic conditions and encouraged the hope that they might help to solve contemporary economic and social problems. Moreover they blended with the popular currents of scientific and evolutionary thought that were undermining the pedagogical foundations of the established economic and social creed and challenging the prevailing theological control of the college curriculum [28] [11, pp. 43-44].

In these circumstances it was inevitable that hostile critics should interpret the strong religious and ethical tone adopted by some of the new-school economists as a sign of soft-headedness and a return to less scientific thought.[11] Ely was indubitably the chief offender; but the early works of J. B. Clark and H. C. Adams (whose names were frequently linked with Ely's by the critics), E. B. Andrews, C. D. Wright, and other prominent members of the American Economic Association also bore the indelible imprint of their religious convictions and moral purpose; and by the late 1880's it was a commonplace of popular literature that economics must, or indeed had already become an "ethical science."

It is therefore no coincidence that the Association attracted considerable support among the liberal clergy,[12] for liberal theology and a progressive attitude to social policy were often found in conjunction with one another. Some of Ely's correspondents praised his "brotherly sentimental" economics and likened the work of the American Economic Association to that of the Home Missionary Society[13]; but it is understandable that orthodox economists like Laughlin, Taussig and Hadley wondered whether the organization was merely substituting one brand of partisan advocacy for another, more pernicious one.

When Hadley protested that the "so-called moral reaction" against orthodox economics was really an "emotional" one, he was not only

[11] W. G. Sumner's characteristically outspoken reference to "the whimsical people who have hobbies of one sort or another, and who cluster around the Social Science Association, come forward with projects which are the result of a strong impression, an individual misfortune, or an unregulated benevolent desire, and which are therefore the product of a facile emotion, not of a laborious investigation" [39, p. 305] may have been typical of the hard-headed conservative viewpoint. Cf. infra, n. 14.

[12] The Association's first published membership list, dated March 1886, included the names of 23 clergymen in a total of 181 members. The number rose to a peak of 39 by 1894 (when the total had reached 800). This is far below the 60 claimed by W. D. P. Bliss in 1892 [36, p. 9].

[13] [14, H. F. Craven to Ely, Nov 11 1902; J. B. Sewell to Ely, Dec 30 1890]. The Ely papers include many such eulogistic references to his moral and religious influence. For a modern opinion see [8, Ch. 4].

expressing his irritation at the arrogant assumption of "a superior moral purpose" on the part of those ". . . trying to right visible wrongs by direct state action," and his belief that ". . . the harm which has been done by laws based on unemotional reasoning is but a drop in the bucket compared with that which has been done by laws based on unreasoning emotion"; he was also endeavoring to defend the economist's scientific reputation.[14] Like his less conservative colleagues, Hadley was only too familiar with the poverty, insecurity and vulnerability of the little band of professional economists [15, pp. 94-95], and he was convinced that prestige and influence could be earned only by exercising sound scholarship and wise statesmanship. When he asserted that the ". . . economists as a body . . . strongly disapprove the attempt to 'popularise' economics by giving too much weight to the conclusions of uninstructed public sentiment" [26, p. 191] he was voicing an opinion that was then (i.e., 1894) shared by most of his fellow members. Since the mid-1880's there had been a noticeable shift in the prevailing tone of American economics, and this shift cannot be fully understood without a close examination of the circumstances underlying the change of leadership in 1892.

III. *The Ascendancy of Science*

During the first seven years of its existence, the leadership of the American Economic Association rested continuously in the hands of Walker as president and Ely as secretary; and the election of Dunbar[15] to the presidency has usually been regarded as a turning point in the organization's development. The transfer of leadership not only indicated that the original fear of capture by the forces of reaction had diminished; as a conciliatory gesture to a prominent representative of the old school it also constituted an armistice that marked the end of hostilities between the two warring schools and the beginning of a

[14] [26, pp. 186-87]. For other conservative reactions to "emotionalism" see [13, p. 24] [30, p. 2]. It was then, and indeed still is, a central tenet of orthodox methodology that analysis and prescription should be rigidly separated, and this was one of the points of difference between the old and new schools. But it is risky to generalize about their respective attitudes to science. Walker and Patten, for example, repeatedly emphasized the distinction between analysis and policy recommendations, whereas Clark, who steadily became more conservative, was always criticized for his failure to make this distinction. There was a key debate on this issue, and on the general question of the social role of the scholar, following Hadley's presidential address in 1899 [25, pp. 62-88].

[15] Dunbar had written a moderate critique of the new school in the opening number of the *Quarterly Journal of Economics* [13], of which he was the editor. He had been invited to join the AEA by Walker and Ely [37, Ely to Seligman, Apr 25 1887] and at a council meeting the next month it was agreed that a deputation should confer with Dunbar about his enrolment. Dunbar (and Hadley) had been present at the Boston meeting in May 1887 and he joined a year later [3, Seligman to Ely, Nov 21 1888; Dunbar to Ely, Dec 12 1888].

period of agreement on fundamentals and renewed emphasis on theoretical work.

Although correct in its essentials, this version tells only part—and, from the present point of view, not the most important part—of the story. Ely's resignation as secretary in 1892 was the immediate outcome of a dispute which marked the effective repudiation of the Association's original commitment to quasireligious social reform; it was accompanied by his decision to move to the University of Wisconsin, a move that symbolized the westward shift in the centre of gravity of American economics; and it was followed, later in the decade, by undercurrents of discontent in the academic ranks which threatened the unity and stability of the American Economic Association. These are matters of more than parochial interest, for they enable us to discern hitherto undisclosed links between the new, German-inspired, school of economics and the progressive reform elements in American institutionalism. Cf. [22] [21, Ch. 7].

As we have seen, there was nothing novel about the desire to widen the Association's representation among the more orthodox economists. There had been a steady if slow influx of such members from 1887 on, due both to individual decisions and to collective efforts at recruitment.[16] The initial sense of insecurity had soon passed, and as early as 1887 Walker had declared his willingness to stand down from the presidency if it would serve the cause, although Ely privately expressed his fears of the continued danger of control by the Yale and Princeton men in 1888, when he sought to persuade H. C. Adams to take over some of the secretarial duties.[17] The dispute of 1891-92 was,

[16] Hadley and Laughlin did not accept early invitations to join the council [37, James to Ely (copy), Apr 27 1887]. Subsequently Taussig wrote to Seligman that Laughlin "intimates that he will come in if Ely goes out" [37, May 30 1892]. In fact Laughlin did not join until 1904, although as late as 1898 efforts were still being made to persuade him [3, Horace White to W. Willcox, May 25 1898].

There had been several attempts to include "the Yale circle" [37, Clark, and Ely to Seligman, Apr 25 1887]. Sumner, the most dogmatic exponent of orthodoxy, presented the most delicate problem. It would be ". . . unworthy of us . . ." Clark wrote to Ely, either ". . . continuing on 'a platform of opposition to Sumner' as one of his close friends called it in conversation with me . . . [or] making undue overtures to him. This would also be making too much of him. He is a prominent man; but if he is a great man I have never done him justice. He seems to me to be chiefly what Pres't Walker once called him, a cantankerous man . . . A few could assure him that he would be welcomed—provided that the fact is that he would be;—but for the executive committee in their official capacity to invite him seems to me to be too far to go . . . I know, of course, that he feels excluded, and would not join without some unusual encouragement. That encouragement had better limit itself to private assurances; and these would have to be preceded by some mutual expressions of view. The case of Professor Dunbar is not in favor of more official invitations" [14, May 30 1889].

[17] "Some seem to think that all things considered, I ought to keep the place and that the division of the office as I wanted is not desirable just now. I have heard a hint, I may tell you in confidence, about a combination of Yale, Harvard and Columbia to capture

however, only indirectly connected with this process, for it was occasioned by Ely's decision to hold the next annual meeting at the Methodist summer camp site at Chautauqua, New York, a decision which brought a chorus of protest from some of the leading members.

There were various reasons for this reaction: the belief that Ely had acted unconstitutionally in forcing his decision on an unwilling executive committee; the suspicion that he was putting his personal interest in Chautauqua before the welfare of the organization, since it was feared that the Association's scientific reputation might be endangered if it became too closely associated with a popular religious educational movement;[18] and finally, the prospect that it would deter the leading representatives of the old school, several of whom had joined but had not yet participated in the Association's activities [19, Seligman to Farnam, June 2 1892]. In the event, the dispute was patched up. Although it was too late to alter the venue of the meeting, which had already been announced in the press,[19] opposition to it was moderated by the knowledge that a public row would harm everybody, and by the fact that Ely, who had at first threatened to resign instantly if the decision were overruled, had agreed to submit his resignation at the next meeting.[20]

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the thing and run it, but that would mean ruin. The West and the South would never submit. Apart from that, a large proportion of our members support us because they suppose we stand for something positive; and were [word illegible] a little because we dropped our 'Statement of Principles'. Now if the choice of Secretary should seem to indicate a further reaction, there are intimations of a split. This would be a great pity" [14, Ely to Adams, Dec 17 1888]. Ely's fears might be dismissed as wildly exaggerated but for the fact that his prophecy almost came true six years later. *Infra*, pp. 568-569.

[18] For Ely's Chautauqua activities see [17, pp. 79-87]. Among those who opposed Chautauqua George Gunton complained that "it is very much like having it held at some spiritualist camp-meeting" [37, Gunton to Seligman, Dec 26 1891]. Similarly Davis R. Dewey wrote that the objectors felt that "Chautauqua is not associated with the highest academic scholarship" [14, Dewey to Ely, Jan 10 1892]. Walker, in referring to a "serious and determined protest" from F. H. Giddings, Taussig and Seligman [and later H. C. Adams], added that he personally would like to visit "the great intellectual camp meeting of the country" [14, Walker to Ely, Dec 18 1891].

[19] There is evidence that Ely had "jumped the gun," [37, Walker to Seligman, Dec 10 and Dec 19 1891] [14, Giddings to Ely, Jan 1892].

[20] "To my request for a call to the Executive committee or of the council," Walker wrote to Seligman "he replies that this can only mean a purpose to humiliate and insult him. and that he will straightway resign.

. . . . . .

"Small loss, you say; and I don't disagree with you. The time passed, three years ago, when Ely had anything to give to the Association. Since then he has been a drag on the Association. But he promises to resign peacefully in August, whereas if he goes out now there will be a specious row. All the Socialist and semi-Socialist papers will join in attacking the Association and raising Ely to the rank of a martyr. The whole Chautauqua influence will be invoked against us. The newspapers will take the affair as 'another row among the political economists' " [37, Dec 23 1891].

Clark agreed: "President Walker is right in deprecating the row I am sure. It would

Some members of the Association undoubtedly interpreted this outcome as a victory for the old school rather than a sign of the achievement of unity. Nor was this interpretation confined to those dissatisfied with the change. In a final, unsuccessful effort to persuade W. G. Sumner to join, his Yale colleague H. W. Farnam gave a vivid glimpse of the Chautauqua meeting:

> This is the first time Yale has been represented at one of these meetings by one of its professors and I think that it created a good impression to have one of them there. At any rate they made me one of the Vice-Presidents.
>
> . . . . . .
>
> There was in general quite a new deal in the officers. Dunbar was made President; a young man by the name of Ross of Cornell was made Secretary and though Ely was consoled by the office of Vice-President, this means practically the end of his régime in the association. In fact it was rather amusing to see on how many occasions he found himself in a minority of one.
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> . . . . . .
>
> In view of these changes do you not think that you would like to join? . . . I think that you would find yourself quite in sympathy with the present spirit of the society. [19, Sep 5 1892].

There was indeed some justification for the dissatisfaction of such men as Ely, E. W. Bemis, and J. R. Commons, to whom the adoption of a more scholarly and "scientific" stance entailed a withdrawal from active participation in economic and social reform, and a growing emphasis on theoretical problems.[21] Yet there is no reason to conclude that the change of leadership, and the subtle shift of policy implied therein, was the outcome of the sinister machinations of a few diehard reactionaries, for it evidently reflected the prevailing tone of professional opinion. When the trend of American economics is seen in its social context, it becomes clear that the preoccupation with marginal utility and marginal productivity analysis in the 1890's did not merely reflect the dissatisfaction with the unfruitful methodological and doctrinal controversies of the previous decade; it also reflected the economists' yearning for scientific status and prestige. This they sought to attain by dissociating themselves from the past, and by establishing economics as an independent scholarly discipline, free from theological, ethical, historical, and sociological connotations and, above all, free from the taint of missionary zeal and political partisanship.

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mean a fight with all that would, in that event, range itself on Dr. Ely's side, and that would be a great deal" [37, Clark to Seligman, Dec 30 1891].

[21] In part the development of a more detached and cautious approach reflected the growing awareness of the complexity of current problems and the inadequacy of simplified solutions. Yet this had usually been the "conservative" attitude [cf. 39, p. 306]. For a recent comment on this see [38].

One of the by-products of this process was the emergence of sociology and economic history as separate disciplines both in the United States and Europe, a process to which such prominent members of the American Economic Association as F. H. Giddings, E. A. Ross, A. W. Small, and W. J. Ashley made notable contributions. On a sociological plane, the change in the character of the Association constitutes one element in what has recently been called the "status revolution" [27, pp. 149-55]—a movement that brought with it a rise in the prestige of the professors and a decline in the status of the clergy. By facilitating the secularization of their discipline the economists were also—not always unwittingly [34]—enhancing their professional status.

III. *Midwestern Academic Discontent*

Yet beneath the superficial harmony resulting from the collective desire to minimize controversy on methodological and policy questions there were significant notes of discord. By far the most interesting and the most neglected aspect of this situation is the emergence of a distinct regional pattern of opinion reflecting contemporary political, social and intellectual tensions which was aided by the expansion of economic training in the middle west. It is obviously impossible to consider here the problems of analyzing regional variations in social and cultural conditions and of tracing their effects upon the work in particular university disciplines.[22] But as the economists could hardly avoid discussing such contemporary issues as free silver, the trusts and the tariff, matters on which there were certain clearly marked regional differences of interest, it is no surprise to find that their attitudes sometimes reflected the pressure of public opinion. The selection of faculty was influenced by local prejudices, and the number of summary dismissals and "academic freedom" cases in the 1890's made the penalties of a defiance of local sensibilities painfully clear [29, pp. 413-45].

Of course these incidents reveal that at least some economists openly resisted local prejudices, and others were doubtless protected by their own tact or by the courage and good will of their university authorities. But as the opinion spread that the American Economic Association had become more conservative, there was a swelling chorus of dissatisfaction from various mid-western centers at a time when radicalism was rife in that region, and this dissatisfaction gave considerable concern to the organization's officials, especially when the threat of a rival body appeared in 1895 with the formation of the Political Science Association of the Central States.

[22] For one interpretation of the differences between eastern colleges and midwestern state universities see [33, pp. 108-17] [cf. 23, pp. 21-27].

The proposal to form this association appears to have originated with John R. Commons and his colleagues at the University of Indiana during the summer of 1894,[23] and it undoubtedly reflected a fairly widespread feeling that the existing national organizations of economists and historians were indifferent to the needs of middle-western scholars. The fear that the new body might become a serious rival to the American Economic Association seems to have been inspired less by the danger that it would be dominated by the ". . . Ely-Commons faction . . ."[24] than by the possibility that it might be captured by the predatory powers of the University of Chicago, an institution which was expanding rapidly with the aid of John D. Rockefeller's fortune and which was widely regarded as a hostile and ruthless rival to the established leaders of the academic world.[25] However, the determined efforts of J. L. Laughlin and H. E. von Holst failed, and the new organization was launched under the leadership of men whose first act was to pass a resolution ". . . distinctly affirming it as the policy of the Political Science Association to arrange its future meetings so as to avoid conflict with the two national organizations in which we are all vitally interested."[26]

[23] [1, Commons to Adams, Jan 9 1895]. G. W. Knight, the secretary, assured Adams that *"It is not in any way a University of Chicago organization* or machine, nor did the call for the conference originate with them" [Jan 8 1895, italics in original]. J. H. Canfield, Chancellor of the University of Nebraska, also claimed authorship of the scheme [3, Canfield to J. W. Jenks, Jan 21 1895].

[24] [1, Adams to Jenks, Jan 7 1895]. Commons assured Adams that he had no hostile intentions towards the AEA [1, Jan 9 1895]. In a letter to Ely, however, Commons had spoken of an ". . . organization of Western economists . . ." who ". . . would be mainly your students . . .," at least initially. There was a need, he added, for such meetings of sympathizers, ". . . especially now that these forces of opposition seem to be marshalling together" [14, Sep 17 1894].

In a brief but revealing letter to Ely the President, Jesse Macy of Grinnell, Iowa, remarked that the Political Science Association was founded on ideas similar to those prevailing when the AEA was started, adding that the new body must not be ". . . swallowed . . ." by the AEA [14, Apr 19 1895].

[25] Among the economists this attitude was based partly on the current suspicion of business influence and, more specifically, on Laughlin's unwillingness to join the AEA and his independent establishment of the *Journal of Political Economy* at a time when strenuous efforts were being made to consolidate and reduce the number of periodical publications on economics. Taussig, for instance, referred to his and Dunbar's ". . . disgust at this intrusion [i.e., the JPE] into the field. It is a clear case of not having the interests of science at heart; though it may be a question whether this interest is pure and undefiled in the breast of any of us. We all have a concern for our own institutions" [37, Taussig to Seligman, Mar 17 1892]. Similarly [19, Seligman to Farnam, Jun 2 1892].

[26] [1, Jan 8 1895]. Yet the reports diverged somewhat. Whereas Knight claimed that ". . . no one was more solicitous that it should not be or even seem to be run by the Chicago men than those men themselves; especially was this true of Professor Small who, by the by, is a prince of good fellows," Bemis reported that the resolution encountered fierce opposition from Laughlin and von Holst (professor of history at Chicago) and three or four others [3, Jenks to Adams, Jan 8 1895].

The officers of the new association were president: Jesse Macy, Grinnell College, Iowa;

Although the new body did not prove to be a serious rival of the American Economic Association, it had a direct impact on the latter's policies; and the purposes behind its formation merit consideration partly because they have apparently not been published previously, and also because they reveal the difficulties facing the officers of the national economic association in their efforts to retain its hold over the profession. G. W. Knight outlined the purposes of the organization in a letter to H. C. Adams: to bring together four groups of scholars who could ". . . get together . . ." in no existing association; to cater for many ". . . poor fellows . . ." in the ". . . western region . . . who as college instructors are trying to work and do their best in two or more of these fields, and who are only partially at home in any other one Association"; to discuss the ". . . many questions of pedagogy and methodology in connection with these fields, and *peculiar to the institutions of this section of the country,* or at least away from the sea-board . . ."; and to give opportunities to men who would not or could not shine at the national meetings [1, Jan 8 1895].

It is clear that the new body succeeded in focusing attention on the westerners' grievances. Although J. B. Clark and J. W. Jenks, as president and secretary of the American Economic Association, did not at first regard it as a serious rival of their own organization,[27] H. C. Adams, who was invited to head the economics section of the Political Science Association, was much less sanguine. Influenced both by his position as head of the economics department in what was widely regarded as the leading state university, and by his scepticism towards recent theoretical tendencies,[28] he sympathized with the motives behind the new venture, and in a letter to J. B. Clark he frankly criticized the economic association's handling of the situation.

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vice-presidents: A. Small (Chicago, sociology), C. H. Haskins (Wisconsin, history), Adams (Michigan, economics) and J. Woodburn (Indiana, political science); treasurer: F. W. Blackmar (Kansas, economics); secretary: G. W. Knight (Ohio, economics).

[27] [1, Jenks to Adams, Jan 8 1895; Clark to Adams, Jan 10 1895] [3, Clark to Jenks, Jan 12 and Jan 21 1895]. Yet Clark's initial response was "The West is stirred up. . . . One thing is clear:—we must make our association so good, and so fair in its policy, that there can be no ground for leaving it, however many rivals enter the field" [3, Clark to Jenks, Jan 10 1895].

[28] Adams was at the University of Michigan, and was also chief statistician to the Interstate Commerce Commission. There are many examples of his attitude to theory: "To my mind the Austrian School has already exhausted itself and I am wondering whether so clear a man as Clark will be able, after committing himself to the mechanical reasoning of the School, [to overcome] its limitations" [1, Adams to Seligman, Jun 1 1896]. Also [1, Adams to Seligman, May 14 1902; Adams to K. Coman, Feb 6 1897]. However Adams recognized his own limitations as a theorist. Of Clark he admitted: "I hardly feel competent to follow him in his later development, but so far as I can understand him, I am obliged to dissent from many of the conclusions that he thinks so pertinent" [1, Adams to C. P. Emerick, Feb 5 1897].

> You ask what I think of the plan of holding in alternate years sectional meetings of the Association. To answer definitely, I do not think it would do at all, that is, it would not serve the purpose we of the West had in mind when insisting that the American Economic Association should meet West of the Allegheny mountains. Perhaps the thing we aim at cannot be attained, that is to keep the A.E.A. to represent the economists of the U.S. This is a very big country and travelling is of course burdensome. But I think the men in the East show altogether too much disinclination to come West. Certainly if it is impossible to have good meetings in any of the western cities, at which a great majority of the members of the East shall be in attendance, we might as well face the issue at once and allow that there shall be three or four economic associations in the country. Of course I shall go with my section if anything of this sort seems to be inevitable.
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> . . . . . .
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> [The Political Science Association] . . . need not be a rival of the A.E.A. . . . I am quite confident, however, although I have no direct evidence of the fact, that it was intended by those who have been responsible for starting it, to break off from the AEA [1, Jan 7 1895].

Some weeks later, after considerable discussion, it was decided to hold a joint meeting of the two associations at Indianapolis. The location is significant, not only because of its proximity to the birthplace of the Political Science Association, but also because the University of Indiana, and Commons in particular, were under severe attack in the state legislature, and it was felt desirable to show ". . . the people of Indianapolis and of the state the determination of the economists of America, no matter what their opinions, to insist upon the freedom of instruction and investigation."[29] At this meeting H. C. Adams was elected president of the American Economic Association, a move that, while fully justified by his standing in the profession, was no doubt influenced by the desire to conciliate the westerners.[30]

IV. *Recession, Revival, and the Attainment of Unity*

Thus the crisis passed, and the American Economic Association records contain no further evidence of rivalry with the Political Science Association. But despite continued efforts to meet the needs of western members,[31] the organization was hardly thriving, and its failure to

[29] [1, Commons to Adams, Apr 6 1895]. Also [3, Clark to Jenks, Jan 21 1895]. Commons was subsequently forced to resign [9, p. 285] although Adams had strongly defended him in a letter to President J. Swain of the University of Indiana [1, Aug 22 1894].

[30] The selection committee comprised Taussig (Chairman), Farnam, R. P. Falkner (Pennsylvania), Ross (Stanford) and W. A. Scott (Wisconsin).

Recent research has tended to enhance Adams' reputation [2, pp. 3-55] [35, Ch. 7].

[31] From 1898 the annual meetings were recommended in the *A.E.A. Handbook* partly because they ". . . counteract any tendency to particularism which geographical separation and the diverse traditions of American colleges might be deemed to foster." In this issue a geographical distribution of members was included for the first time.

expand caused serious concern between 1894 and 1899, the period immediately preceding Ely's presidency.[32] There was a significant shift of emphasis which may have reflected the belief that ". . . political economy is swinging back to a renewed attention to practical or business affairs."[33] Although there appears to have been no serious effort to recruit new members until 1899, the 1896 *Handbook* made a general appeal for recruits, and from 1894 onwards the introductory description of the Association's character and purpose increasingly emphasized the width of its support among business and professional men.[34] In 1899, as an outcome of this tendency, there was a serious examination of the desirability of electing a businessman as president in succession to A. T. Hadley; but this proposal was rejected, partly because it would have represented too sharp a reversal of the former emphasis on the scholarly character of the Association. As J. B. Clark put it,

> . . . we should remain a scientific body. . . . At bottom even the phillistines will have more respect for such a body than they would for one that should put a man of affairs at its head. I may be wrong, but I dread any yielding to the view that economic wisdom resides outside of the schools and inside of the counting house [3, Clark to C. Hull, Dec 23 1899].

As it transpired, Ely's long-delayed return to the fold, followed by his unexpected election to the presidency,[35] provided a happy, if tem-

[32] In 1894 the treasurer complained that membership was far below that of the American Statistical Association [3, F. B. Hawley to Jenks, Nov 5 1894]; and five years later Seligman (then treasurer) informed Willcox (then secretary) that the membership of the American Historical Association was both higher (1200) than that of the AEA, and increasing, adding that ". . . if we once cease to advance the crisis is not far off" [3, Feb 15 1899]. Other letters of 1899 refer to the "crisis" and the need for "careful attention and nursing." Membership had fallen from 781 in 1893 to 661 in 1894; and had only risen to 685 by 1898 and 745 by late 1899.

[33] [24, p. 55]. Giddings called on members ". . . to take a more prominent stand on questions of public policy" and ". . . to make the A.E.A. an increasingly powerful influence upon public opinion . . . we must not impose upon ourselves a creed, or promulgate dogmas; but we should make it clear to the people that there is a vital difference between scientific and unscientific views of those great subjects" [24, p. 56 sentence order altered].

[34] Early in 1899 a circular was drafted for prospective members including the following phrases from Hadley's 1898 presidential address: "If we fail in our influence in public life we fail in what is the most important application of our studies . . . The largest opportunity of our economists in the immediate future lies not with students but with statesmen. . . . [In future the Association] will be of interest to thoughtful businessmen, newspaper men and holders of public office" [1, Willcox to Adams, Feb 16 1899]. Somewhat bluntly it was added that the limits of college membership had been reached!

[35] Although Ely had presided at the 1893 meeting in Chicago, he had subsequently held himself aloof until 1899, when he offered a paper. Indeed, he even allowed his membership dues to lapse for "5 or 6 years" [3, Willcox to Giddings, Dec 2 1897]. At the 1899 meeting Hadley, the president, selected him as chairman of the nominating committee on the assumption that in view of his recent absences he would hardly be a candidate for office. But, to the secretary's surprise, he was elected president [3, Willcox to S. M. Lindsay, Jan 2 1900; Willcox to Hull, Jan 6 1900].

porary, solution to the dilemma. Whatever the estimate of Ely's scholarship, his inexhaustible energies and remarkable administrative gifts were enlisted at a most opportune time. Under his businesslike guidance and largely owing to his personal efforts, membership expanded sharply,[36] interest revived, and the Association prospered.

Yet complete unity was not attained until 1904 when, following a University of Chicago decision to invite the historical and economic associations to a joint meeting in their city, J. L. Laughlin at last applied for membership.[37] Trivial as this incident may now appear, it seemed a matter of some importance to the officials of the American Economic Association. It dispelled the fear that Laughlin might use his considerable influence as head of the Chicago department of economics to spread distrust and hostility towards the Association,[38] and by adding the support of the only contemporary economist of note who had hitherto held aloof, it enabled the economists to present a united front in their efforts to enhance the public prestige and influence of their professional discipline. In retrospect it is clear that Laughlin's action marked the final disappearance of the suspicion of the organization which it had encountered, in varying degrees, since its inception, and betokened its permanent establishment as a strictly scientific and scholarly body.

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[36] Membership rose from 745 in Dec 1900 to 950 in Dec 1901 during Ely's term of office, and to a peak of 1011 in 1902. Thereafter it remained at about this level until 1909, when the then secretary T. N. Carver conducted another membership campaign.

Of Ely's presidency Hull, the treasurer, wrote to Taussig: "I think that Ely, too, has the idea of distinguishing his administration by adding to the membership of the Association, and to judge from his initial success as Secretary, I fancy his design is not altogether hopeless of execution" [3, Mar 8 1900]. At the conclusion of Ely's term of office Hull wrote: "I feel that we really owe more as an organization to you than to any other President whom we have had since I have known anything about our affairs" (i.e., since 1893) [14, Hull to Ely, Nov 22 1901]. Similarly [14, Fetter to Ely, Jan 11 1902].

[37] [14, Small to Ely, Jan 10 1904]. Jameson, secretary of the American Historical Association and professor of history at Chicago, spoke of Laughlin's genuine cordiality towards the AEA and the difficulty ". . . of persuading several scores or hundreds of members that his attitude is and would be a cordial one" [3, Jameson to Seligman, Nov 29 1902].

[38] However unjust to Laughlin, this feeling was real enough. Wesley Mitchell, then a promising young man, was especially invited to read a paper in 1902 in an effort to protect him from "bad influences" at Chicago [3, Fetter to Seligman, Oct 18 1902; Willcox to Hull, July 23 and 27 1900].

The abolition of the council, proposed in 1904 and effected in 1905, was a final explicit acknowledgement of the disappearance of the possibility of domination by a sect or group of reformers [15, p. 8].

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OPERATIONS RESEARCH

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Once upon a time, within the memories of men who think they are still young, there was no such thing as operations research, or at least no such phrase. Today the Operations Research Society of America has more than 2,300 members, the Institute of Management Science has more than 2,600 members (there is considerable overlap, of course) and at a recent international conference on operations research no less than sixteen countries from four continents were represented. It is a flourishing movement.

The proper way to begin an inquiry into this movement would be to define it. But this is difficult; for operations research is not a subject-matter field but an approach or method. And, even after a study of hundreds of examples of work classified as operations research, it is by no means clear just what the method is other than that it is scientific (like all respectable methods), because operations analysts are typically resourceful and ingenious men who tackle their problems with no holds barred. I propose, nevertheless, to advance a definition; but it will help to prepare the way for that hazardous attempt if I try to convey the flavor of operations research by sketching a pair of contrasting caricatures, one of a conventional business consultant and one of an operations analyst.

Suppose that a soap company seeks advice as to whether its advertising budget next year should be larger than, the same as, or smaller than this year. They might engage a business consultant who, in this case, would be a specialist in either advertising or marketing. He would have had substantial experience with advertising the same or similar products and would have at his finger tips a good many relevant data and, besides, would be familiar with all the standard sources of such data. In addition he would be aware of the maxim that it takes five cents worth of advertising to sell a dollar's worth of soap, though he would not necessarily take it very seriously. With this background at his disposal he would marshall the pertinent facts. He would examine the experience of his client and would correlate sales with advertising

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expenditures for past years, doing it by eye if his research budget were stringent or by least-squares if he anticipated a fee that justified a more imposing report. He might well consider data by states or marketing areas, for both his client and competing firms, and might also analyze sales and advertising exposure broken down by city size. If the project were at all large he would almost certainly have a field survey made, finding out from a random sample of housewives what soaps they bought and what advertisements they were aware of. Finally, with this information all carefully organized, he would determine the level of advertising expenditure appropriate to the budgeted level of sales. In all likelihood this figure would turn out to be fairly close to 5 per cent of anticipated gross sales, partly because you can't go far wrong by sticking close to an established norm, and partly because nearly all his data will be in this range with the exception of a few observations relating to attempts to launch brands that, for one reason or another, did not catch on, or relating to advertising campaigns that had been successful beyond reasonable hope of duplication. In short, he would arrive at a recommendation based on a reasoned scrutiny of past experience.

But now suppose that the soap company turned to an operations analyst for advice. Typically, this man or firm would have no particular experience with either soap or advertising, and would feel the need of none. More likely than not, the analyst would be a graduate physicist or mathematician. With this background at his disposal he would formulate the problem. The first words he would say to himself might be: "Let p_{xt} be the probability that the xth household will buy my client's soap during the tth week.[1] Then p_{xt} is the product of two factors: p'_{xt}, the probability that they will buy any soap during the week, and a_{xt}, the probability that if they buy any soap it will be my client's brand. Assume that p'_{xt} cannot be influenced by my client's advertising but that a_{xt} can be. In fact, suppose that the Weber-Fechner law applies so that the rate of increase of a_{xt} with respect to advertising expenditure is inversely proportional to the level of advertising expenditure already attained, i.e.,

$$\frac{da_{xt}}{dE} = \frac{c}{E}$$

where E is the level of advertising expenditure and c is a constant of proportionality. Then, integrating this differential equation, $a_{xt} = \log kE^c$ where k is a constant of integration to be determined from the data. Then $p_{xt} = p'_{xt} \log kE^c$ and total expected sales can be estimated by integrating this expression over the entire population (i.e.,

[1] In this and most later examples the mathematical details are not essential to the general discussion and may be skimmed.

all values of x). Thus, assuming p'_{xt} and k to be given data, total sales can be estimated as:

$$s_t = \log kE^c \int p'_{xt} dx$$

and

$$\frac{ds_t}{dE} = \frac{c}{E} \int p'_{xt} dx.$$

Thus the optimal level of advertising can be found by finding the value of E at which the profit per dollar's worth of sales multiplied by this derivative equals 1, the cost of an additional dollar's worth of advertising."[2] In short, he would arrive at a recommendation based on logical deduction from simple first premises which are a plausible approximation to the laws underlying the phenomenon being studied.

Each of these approaches is "scientific" according to its own canons, but they are quite different. We can characterize this difference by saying that the operations analyst, in contrast with the conventional business analyst has a strong predilection for formulating his problems by means of formal mathematical models. By a model I mean a symbolic description of a phenomenon in which its observable characteristics are deduced from simple explanatory first principles (i.e., assumptions) by manipulating symbols in accordance with the laws of some formal logic (usually ordinary mathematics).

Argument *ad hominem* is generally regarded as unscholarly, but in trying to characterize operations research it seems important to note who the operations analysts are. According to a survey conducted by the American Management Association [2], more than 40 per cent of operations analysts are engineers by training, another 45 per cent are mathematicians, statisticians, or natural scientists. It is only natural that the point of view in which these men are schooled should permeate operations research. The essence of this point of view is that a phenomenon is understood when and only when it has been expressed as a formal, really mechanistic, quantitative model, and that, furthermore, all phenomena within the purview of science (which is probably all the phenomena there are) can be so expressed with sufficient persistence and ingenuity. A second characteristic of men of science, amounting to a corollary of the first, is their preference for symbolic, as opposed to verbal, modes of expression and reasoning. These characteristics I take to be the style of operations research, and I define operations research to be all research in this spirit intended to help solve practical,

[2] In constructing this fable I have followed the spirit of the only operations research study of advertising with which I am acquainted, but have carefully diverged from that study in all details.

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immediate problems in the fields of business, governmental or military administration or the like.

There is an important corollary to the tendency of operations analysts to cast their thinking in terms of formal mathematical models. Operations research is not a descriptive science but a prescriptive one. Therefore the deduction of adequate descriptive models is only part of the task of the operations analyst. In the end he must come up with a recommendation for action and this requires that he know what the operation in question is intended to accomplish. We rather side-stepped this issue, in the fable at the beginning of this essay, by assuming that the objective of the advertising was to attain the maximum possible excess of gross sales over the sum of production costs and advertising expenditure. But the conscientious operations analyst does not so glibly assume the objective of the operation he studies; and the extensive literature devoted to studying the objectives of business enterprise shows that he is wise to be circumspect about this point. In the soap example, it may well be highly important to maintain total sales at a level that makes it worth while for retailers to stock the brand in question, even if the marginal net return is negative. The long run may have to be considered, if the brand has to establish or maintain a market position in the face of vigorous competition. In short the objectives of an operation are likely to be complicated, obscure, and composed of several incommensurable aspects. A major part of the task of the operations analyst is to construct a "measure of merit" for the operation he studies to accompany his formal description of it. The logical precision of the model enforces corresponding precision in expressing the objectives that the operation is intended to attain. The orthodox business consultant, on the other hand, is under no such pressure to formulate precisely the goals of the enterprise he studies.

When the operations analyst has formulated the model of his undertaking and the goals it serves he is still nearer the beginning of his analysis than the end. He must still particularize his model by estimating the values of the various "given" parameters that enter into it. For this purpose he employs, usually, more or less advanced statistical methods. Then he must solve the model, that is, find explicit relationships between the parameters under the control of his client, on the one hand, and the measure of merit on the other. When this has been done he is in a position to determine the optimal values of the decision parameters, to make his recommendations, and to try to persuade the management to adopt them.

In the next two sections we shall discuss a number of aspects of the problem of model formulation. Then we shall examine some of the problems and pitfalls of determining measures of merit or objective

functions. The concluding section is devoted to the conditions for successful operations research and to some general conclusions and remarks.

I. *Some Standard Models*

There are three aspects to model building, closely related to each other but requiring different skills. The first is straightforward description; expressing the situation under study in terms of the symbolism adopted. This involves inventing symbols for the various components of the system and writing down the relationships connecting them. Usually the symbolism adopted is algebraic and the basic relationships take the form of equations and inequalities. Frequently, though, block diagrams are used. Then the components are represented by labeled boxes and the relationships by lines connecting the boxes. Less frequently, a logical symbolism is employed, with the elements of the problem conceived of as classes or members of classes and the relationships being those of class inclusion, overlapping, and the like. In any event, the process is one of translating the real world situation into a set of abstract and simplified symbols. The result is an essentially tautological and sterile description of the problem, for it yields almost nothing more than definitions and identities.

The second stage we can call creative hypothesizing. This is the stage at which the motivational, behavioral, and technological assumptions are introduced. It entered the soap example in two ways. First in the selection of relevant variables, as when we decided that the price of the soap, competitors' advertising expenditures, the level of national income, the season of the year, and many other conceivable variables could all be omitted. Our fictitious operations analyst made the same kind of conjectural judgment when he decided that the probability that a household's total purchases of soap of all brands was uninfluenced by his client's advertising. But the most vigorous exercise of creative hypothesizing occurred when he conjectured the form of the relationship between advertising and the conditional probability that any soap purchased would be his client's brand. The first aspect of model formulation is a craft but the second is an art and introduces the crucial assumptions of the model.

The final aspect of model formulation is quantification: the assignment of numerical values to the parameters of the model. In the soap example these parameters are c and p'_m, the latter being a different number, presumably, for each class of household. This last aspect is clearly a more or less involved problem in statistical estimation.

It is clear that the operations analyst works, basically, as an applied mathematician with some statistics and numerical analysis thrown in.

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His principal tools, aside from his own judgment and imagination, are algebra, the calculus, differential equations, probability theory and statistics. In addition he has some special-purpose tools of his own.

In principle, each time a problem is referred to an operations analyst he could construct a tailor-made model to fit the case, and in practice he does so a large proportion of the time. But fortunately problems of essentially similar form arise repeatedly in widely differing contexts. Consider, for example, the problems of providing facilities for checking customers out of a supermarket, accommodating aircraft arriving at an airport, servicing locomotives at a repair depot, and meeting the needs of telephone subscribers. In each case the problem is to provide tolerably prompt service to a demand whose timing cannot be predicted exactly, by means of expensive facilities. This problem, in all its variants, is the problem of queuing theory. Similarly, the problem of allocating limited resources among a number of uses pervades businesses and administrative organizations of all kinds. This family of problems is the concern of programming, linear and otherwise. And in like manner, game theory, inventory theory, servo-mechanism theory all study types of problems that are likely to occur throughout a wide variety of business and administrative circumstances. The operations analyst has at his disposal a large and growing kit of such tools and is thus provided with an efficient apparatus whenever he recognizes that a particular problem fits into one of these tidy categories. Naturally these ready made models are the most communicable, teachable, and talked about aspects of the craft and tend to receive a disproportionate emphasis both in the public image of operations research and in professional instruction. They will also receive disproportionate emphasis here.

1. *The Linear Programming Model*[3]

Essentially linear programming is a mode of expressing the problem of allocating scarce resources that has the peculiar virtue of lending itself to statistical estimation and numerical solution. It applies when the activities of an enterprise are limited by a number of resources in limited supply and when these resources are to be allocated to a number of activities each of which absorbs them in proportion to its level of utilization. Each of the activities also contributes to the attainment of the objectives of the enterprise in proportion to its utilization. The problem is to discover a set of activity levels attainable within the resource limitations which leads to the maximum possible attainment of the objectives.

[3] For a more complete discussion of linear programming from an economist's point of view see Baumol [6] or Dorfman [14].

One of the most frequent and successful practical applications of linear programming is to the scheduling of oil refinery operations.[4] We can convey the flavor of the method by laying out a highly simplified example of this application. Suppose a refinery has 1000 units of blending capacity which it uses to produce regular and premium motor fuel (to be denoted by subscripts 3 and 4 respectively) from two grades of blending stocks (to be indicated by subscripts 1 and 2). Introduce four decision variables x_{13}, x_{14}, x_{23}, x_{24} where x_{ij} denotes the number of barrels of blending stock i devoted to the production of motor fuel j. Now consider the consequences of decisions regarding these four variables. The total input of blending stock 1 is $x_{13} + x_{14}$ and that of blending stock 2 is $x_{23} + x_{24}$. If the two stocks cost \$1.26 and \$1.68 per barrel respectively, the total cost of fuel inputs is:

$$1.26(x_{13} + x_{14}) + 1.68(x_{23} + x_{24}).$$

The total output of regular-grade gasoline is $x_{13} + x_{23}$, that of premium-grade is $x_{14} + x_{24}$. If the regular grade is worth \$4.20 per barrel at the refinery and premium grade is worth \$5.04 per barrel, the value of product is:

$$4.20(x_{13} + x_{23}) + 5.04(x_{14} + x_{24}).$$

By subtraction we find the excess of the value of outputs over the value of inputs to be:

$$2.94x_{13} + 3.78x_{14} + 2.52x_{23} + 3.36x_{24}.$$

We suppose that the objective of the plan is to make this number as large as possible.

Now turn to restrictions on choice. Suppose that the only quality specification to be considered is octane rating. The octane rating of any blend is a weighted average of the ratings of its components. Thus suppose that the octane rating of blending stock 1 is 75 and that of blending stock 2 is 93. Since regular-grade motor fuel is a blend of x_{13} barrels of stock 1 with x_{23} barrels of stock 2, its octane rating is:

$$\frac{75x_{13} + 93x_{23}}{x_{13} + x_{23}}.$$

Similarly the octane rating of the premium fuel is:

$$\frac{75x_{14} + 93x_{24}}{x_{14} + x_{24}}.$$

Now suppose that regular fuel must have an octane rating of at least 82 and premium fuel a rating of at least 88. Then we have the constraints on the decision variables:

[4] See Charnes, Cooper, and Mellon [12] and Manne [28] for more realistic examples.

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$$\frac{75x_{13} + 93x_{23}}{x_{13} + x_{23}} \geq 82,$$

$$\frac{75x_{14} + 93x_{24}}{x_{14} + x_{24}} \geq 88,$$

which are equivalent to:

$$-7x_{13} + 11x_{23} \geq 0,$$

$$-13x_{14} + 5x_{24} \geq 0$$

respectively.

Finally suppose that each barrel of regular fuel produced requires 1 unit of blending capacity, and each barrel of premium fuel requires 1.2 units. Then, since 1000 units are available in all, we have the capacity constraint:

$$(x_{13} + x_{23}) + 1.2(x_{14} + x_{24}) \leq 1{,}000$$

Gathering all these formulas we have the purely formal problem: Find $x_{13}, x_{23}, x_{14}, x_{24}$ so as to make:

$$2.94x_{13} + 3.78x_{14} + 2.52x_{23} + 3.36x_{24}$$

as large as possible, subject to the restrictions:

$$7x_{13} - 11x_{23} \leq 0$$

$$13x_{14} - 5x_{24} \leq 0$$

$$(x_{13} + x_{23}) + 1.2(x_{14} + x_{24}) \leq 1{,}000.$$

The rest is arithmetic calculation.

Besides being of substantial practical importance, this example has a number of pedagogical virtues. In the first place it illustrates the flexibility of linear programming. Note that the numbers we had to choose, x_{13}, x_{14}, x_{23}, x_{24} were "activities" in only a very strained sense. In this context, what we would ordinarily think of as an activity would be blending one of the grades of motor fuel by a specified chemical formula (e.g., blending a barrel of regular fuel by using .6 barrels of blending stock 1 and .4 barrels of blending stock 2). Linear programming cannot take account of more than a finite and fairly small number of activities (say 200) when defined in the ordinary way, but with our particular choice of variables we have admitted to consideration an infinite number of activities, viz., all physically possible blends. The resource scarcities, also, represent resource scarcities in only a very extended sense. The "1000" is the quantity of a genuine limiting resource, but the two zeros of the right-hand side of the restricting inequalities are artifacts resulting from the manipulation of the quality specifications. This flexibility—the fact that the words "activity" and "resource limitation" do not have to be taken at all literally in setting

up a linear programming problem—is largely responsible for the widespread applicability of the method. Any problem that can be expressed by means of linear equations and inequalities is amenable to linear programming, whatever the physical interpretation of the variables and relationships. Indeed there are methods for incorporating nonlinear relationships, at the cost of substantially increased difficulty in computation.

One important limitation of linear programming has always been that the variables whose values are to be determined must be continuously variable, but recently methods have been developed for solving problems where the decision variables must be integers (e.g., the number of aircraft flights per day between two points).[5] Another important limitation, on which less progress has been made, is that linear programming formulations do not allow for uncertainty.

Let us return to the blending example for another remark. A key theorem of linear programming states that there always exists an optimal solution in which the number of decision variables with positive values does not exceed the number of constraints, in this case three. Now suppose, for example, that the positive variables turn out to be x_{13}, x_{23}, x_{24}. This indicates that the premium fuel would be 100 per cent pure blending stock 2. But this cannot be optimal since the quality specification for premium fuel would be overfulfilled and money could be saved by adding in some of the cheaper blending stock. Clearly, no admissible combination of three positive values can be optimal in this problem, so there must be a solution with only two of the decision variables at positive value. This means that only one of the two products is made, i.e., the refinery produces either regular or premium fuel but not both. Which product should be made can be ascertained readily by computing which of the two yields a greater gross revenue per unit of blending capacity absorbed. Thus the arithmetic turns out to be even more trivial than it promised to be at first glance.

An even more common application of linear programming than the refinery blending problem is the so-called transportation problem, which arises whenever a standardized commodity is to be shipped from a variety of sources to a variety of destinations.[6] Newsprint companies use it to decide which of their customers should be supplied from each of their mills, oil companies use it in assigning bulk distribution plants to refineries, the National Coal Board in England uses it to allocate markets among mines, Henderson applied it to an appraisal of the economic efficiency of the coal industry in this country [20]. There has accumulated a large literature which deals with such complications as

[5] See Gomory and Baumol [19] and Markowitz and Manne [30].

[6] For a typical small-scale example see Vazsonyi [39, p. 26 ff].

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differences in production costs at different supply points, limitations on the capacity of shipping routes, fixed charges for opening or constructing shipping routes, and a cascading of problems such as where distribution involves a chain of factory to warehouse (of limited capacity) to retail store.

All the problems in this wide family are surprisingly easy to solve (though I wouldn't recommend that you try a large one with pencil and paper) for a number of technical reasons, among them the fact that all the restraints take the form of simple sums like: for any factory the sum of shipments to all its customers cannot exceed its capacity. As a result transportation problems involving literally thousands of origins and destinations can be solved readily. It seems that even more efficient methods of solution result when the linear programming point of view is abandoned and the problem is conceived of as routing a flow through a network, so that shipments of commodities are analyzed as if they were currents in electric circuits or flows of liquids through systems of pipes.[7] To use electrical terminology, transportation costs are analogous to resistances, and differences in the value of the commodity at different points are the voltages.

2. *Information Theory*

Information theory rests on the discovery of a quantitative measure of information, originally by R. V. L. Hartley and put into its current form by C. E. Shannon [36]. The unit of information is the "bit," which is the amount of information in a message that tells which of two equally likely possibilities is the case. Thus the telegram "It's a boy" conveys (approximately) one bit of information to a receiver who knew previously that a birth had occurred. Or consider a kind of message that is more common in industry: a requisition sent by a production department to a store-room. It contains at least three kinds of information: (1) which item is desired. This selects one of a large number of not-equally-likely possibilities, but its information content is measurable in bits by a formula that we do not have space to explain. (2) The quantity desired—again a selection from a large number of not-equally-likely possibilities. (3) Which department desires the items—again quantifiable in the same way. The formulas for information quantity are constructed in such a way that the information content of the message is the sum of these three components (and any others, such as the date desired, that may be present). Of course, there are many messages whose information content cannot be measured. For example, I do not

[7] The network approach to transportation problems is due mostly to Ford and Fulkerson. See [17] [18].

suppose that the information-content of this essay can be quantified.[8] But equally obviously, the smooth running of any organization depends on the flow of messages that are sufficiently standardized so that their information content can be estimated numerically.

The metric for information content is accompanied by measures of the information-carrying capacity of channels and the information-processing capacity of centers. A center is any person or group who originates messages or receives and acts on them. A channel is any group of devices or persons that transmits messages between centers. E.g., a channel might be typist-messenger boy-receptionist-secretary; or microphone-transmitter-radio waves-receiver-loud-speaker).

The capacity of a center depends on the kinds of messages it processes, the kind of action or decision required, and the technical characteristics of the center. The capacity of a channel depends on the amount of interference present in it (i.e., irrelevant signals, technically called "noise"), the forms of the messages it handles, the rate at which it can transmit the elementary symbols of which messages are composed, its susceptibility to error, etc. The capacities of the physical components of a channel (e.g., electronic amplifiers) can often be estimated from engineering considerations, the capacities of the human components have to be derived from statistical studies.

The relevance of the form of message to the capacity of a channel is particularly significant. It is contained in the notion of "redundancy," i.e., the ratio of the actual information content of the message to the maximum amount that could be packed into a message of the same length (subtracted from unity, of course). Thus the information content of the vital statistic telegram mentioned above is entirely contained in the zero-redundancy message "B" or the somewhat more redundant "Boy." Redundancy of form is expensive, but it is also useful, up to a point. Thus if a channel is subject to error (as all are) redundancy often saves the day. The message "F" conveys no information, but "Foy" is practically as good as the correct message. Mail order companies recognize the virtue of redundancy when they require that an order include the name of the item desired along with the catalogue number. The optimal amount of redundancy in a system, i.e., the optimal form of message, is an important field of application of information theory. For example, the ordinary method of transmitting television signals is more than 99 per cent redundant (because it is practically certain that the spot following a bright spot will also be bright; the only information needed after a few bright spots is when the first dark spot

[8] But an upper limit can be placed on it by using the estimate that text written in English contains about 1.5 bits per letter.

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occurs). One of the more important advances that made color television feasible, since color TV requires much more information per second, was a less redundant scheme for encoding the signals for transmission.

The relevance of information theory to communications engineering is evident but would not qualify it as a model for operations research. Its contribution to operations research derives from its bearing on the structures of organizations. Here the essential principle is that an organization will not function smoothly if the amount of information to be sent on any channel or processed at any center exceeds the capacity of that channel or center. Thus the information structure of an organization can be depicted by representing it as a kind of network with the centers shown by capacitated nodes and the channels by capacitated links. Then the required flow of information between any pair of centers can be compared with the maximal attainable flow.[9] Bottlenecks as well as underutilized channels can then be detected and corrective actions indicated. Possible ameliorative measures are: (a) increasing physical channel capacity (e.g., increasing the number of trunk lines), (b) reducing the redundancy of messages (which is always unduly high in organizations that have not given conscious attention to this problem), (c) routing messages indirectly via underutilized channels. (d) increasing the capacities of centers (e.g., installing more counters in the storeroom), and (e) reassigning functions away from overloaded centers.

3. *General Systems Analysis*

General systems analysis or cybernetics is fairly closely related to information theory and indeed incorporates it. Like information theory, it visualizes an organization in terms of a block diagram with centers connected by lines of communication. The separation of centers entails that no center will have all the information available to the organization (contrast with the ideal entrepreneur or firm of economic theory), and that there will be delays (lags) in transmitting information to centers where decisions are made and in transmitting and executing instructions from executive centers. This circumstances gives rise to the problem of systems analysis: to deduce from the structure of information flows in an organization and the decision rules employed by its various centers how the organization will respond to changes in its environment and conditions. As a corollary, systems analysis is concerned with methods of ascertaining the information flows and decision rules in organizations

[9] From this point of view Paul Revere's communication system (two lanterns, one if by land, two if by sea) was just adequate. There was one bit of information to be transmitted and his channel had a capacity of one bit per night. It was insufficiently redundant, however, and if there had been much noise (mist, gusty winds) the course of the Battle of Lexington might have been other than it was.

—which often differ from those in the official organization chart—and with finding optimal information flows and decision rules.

The fundamental assumption of systems analysis is that all decision rules can be thought of as responses to discrepancies between the actual state or performance of the organization and some "standard" or desired state, and are intended to reduce those discrepancies. Now it has been noted that many mechanisms behave in just this way (the conventional examples are gyro-pilots on ships and thermostats attached to home heating systems) and, indeed, so do the nervous systems of most organisms. Thus automatic control devices, neuromuscular responses, and organizations are all recognized as instances of control by feedback, i.e., by comparison of actual state with desired state. The mathematics and concepts developed for the study and design of electronic control devices thus become applicable to the study of nervous systems and organizations, indeed the "servomechanism" becomes the model employed in all such studies.

Consider a very simple example which evades all the interesting but complicated mathematical concepts that dominate this area of operations research. Suppose a firm has the policy of keeping 100 units of some item in inventory. Each Monday the stock-room reports to the purchasing department its actual stock as of the close of business the previous Friday. If the stock is less than 100 units the purchasing department orders an amount equal to ¾ of the discrepancy, which is delivered 8 to 10 days later. Sacrificing realism to simplicity we also assume that if the stock exceeds 100 units the purchasing department disposes of ¾ of the excess, say by sale to a subsidiary at a sacrifice price. This disposal (or negative delivery) also takes place in the week following the report of the discrepancy. This very bad inventory policy can be portrayed as in Figure 1. This diagram is interpreted as follows.

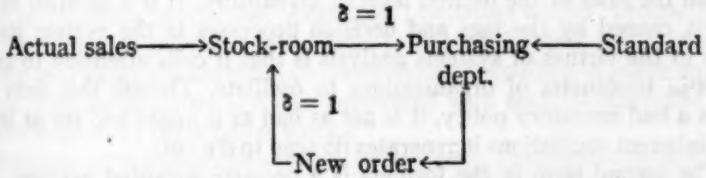

FIGURE 1. FLOW DIAGRAM FOR A STOCK CONTROL SYSTEM

The condition of the stock room is influenced by sales and new orders, the formula being:

$$I_{t+1} = I_t + R_t - S_t$$

where

I_t = inventory at the beginning of the tth week

R_t = replenishment stocks received during the tth week (which may be negative)

S_t = sales during the tth week.

The condition of the stock-room is reported to the purchasing department with a delay to the following week, symbolized by $\delta = 1$. The purchasing department compares this report with the standard (100 units) and issues a replenishment order according to the formula:

$$\text{Order} = {}^3/_4\,(100 - I_t)$$

This order reaches the stock-room the following week and influences the terminal stock according to the inventory balance equation. Now:

$$R_t = \text{Orders in week } t - 1 = {}^3/_4\,(100 - I_{t-1}),$$

so we can write the difference equation for inventories as:

$$I_{t+1} = I_t + {}^3/_4\,(100 - I_{t-1}) - S_t$$

or:

$$I_{t+1} - I_t + {}^3/_4 I_{t-1} = 75 - S_t.$$

This is a familiar kind of difference equation from which we calculate how the level of actual inventories will respond to any pattern of sales. The solution, skipping over the tedious details, is:

$$I_t = k(.866)^t \cos(55t + \phi)^\circ + 1.06 \sum_{\tau=0}^{t-1} (.866)^\tau \sin 55\tau^\circ (100 - {}^4/_3\, S_{t-\tau})$$

where k and ϕ are disposable constants to be assigned in accordance with the initial conditions. As would be expected from a second-order difference equation, this solution displays a combination of exponential and trigonometric behavior. It consists of two terms. The first term is a damped cycle, but the notable aspect of it is that it does not depend at all on the sales or the desired level of inventories. It is a damped oscillation caused by the lags and decision processes in the system itself. One of the virtues of systems analysis is that it calls attention to such built-in tendencies of organizations to oscillate. Though this firm allows a bad inventory policy, it is not as bad as it might be; for at least the inherent oscillations it generates do tend to die out.

The second term in the formula is a peculiar weighted average. To interpret it imagine first that sales were equal to zero for a long enough sequence of weeks so that the transient died out and inventories no longer changed. Then we should have:

$$I_{t+1} = I_t = I_{t-1}$$

and, substituting in the difference equation we should find $I_t = 100$, as

desired. But if sales were steady at any nonzero level, the steady-state level of inventories would be:

$$I_t = 100 - {}^4/_3 S_t.$$

Thus each of the terms in parentheses in the weighted average is the steady-state level of inventories corresponding to the level of sales τ weeks previous to the date for which the calculation is made. The whole term averages these historical steady-states with weights that decline generally as we recede into the past. This term, also, is characteristic of the responses of a feed-back system—their current behavior is a weighted average of responses to past stimuli. The scheme of weights, in this case $(.866)^t \sin 55t°$, is one of the important traits characterizing an organization or mechanism.

Our example was so simple that we determined the inherent oscillations and the weighting function by straightforward, elementary means. In this respect the example was atypical of systems analyses. More generally, fairly advanced mathematics (in particular, Laplace transform methods) are required; and still more generally an explicit general solution cannot be obtained at all, though the formulation without solution may be illuminating. We shall return below to the treatment of problems that defy explicit solution.

4. *Inventory Models*

Churchman, Ackoff and Arnoff state, "More O. R. has been directed toward inventory control than toward any other problem area in business and industry" [13, p. 195], and this may be true. The literature devoted to inventory problems is enormous and even the early work in the field, contributions published thirty years ago and more, displays the mathematical-engineering approach characteristic of operations research. The purpose of all this work, of course, is to determine most-profitable inventory policies. It takes it for granted that inventories are held to facilitate production operations (including within "production" the activities of purely trading firms). Though holding inventories is costly it is generally essential to the economical conduct of other operations. The methods we are now concerned with seek to determine the efficient levels of these buffer stocks.[10]

Because of the enormous variety of inventory problems—style goods vs. standardized goods, goods purchased from outside suppliers vs.

[10] Quite clearly these studies of inventory problems have only slight connection with the aspects of inventory behavior that typically attract the attention of economists. Economists typified by Abramovitz, Hawtrey and Metzler concentrated on the aggregative consequences of inventory fluctuations and on the speculative motives for changes in stocks, with relatively light attention to the more work-a-day motives. Whitin, however, has tried to bridge the gap [40].

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goods manufactured within the firm, goods obtained under conditions of increasing, decreasing or constant costs, seasonal goods vs. nonseasonal ones, predictable vs. unpredictable demand, situations where stock-out penalties are severe vs. those where they are negligible, etc.—there can be no simple, generally applicable body of sound inventory doctrine. Instead, "inventory theory" consists of a battery of techniques for deducing efficient inventory policies adapted to specific circumstances. These techniques can be divided into three classes that I shall call static analyses, stationary-state analyses, and dynamic analyses, in order of increasing sophistication.

Static analyses concentrate on average behavior in a single period considered in isolation. A typical and important result is the "square root law," which can be deduced, in a very simple context, as follows. Suppose that the average weekly requirement for some inventoried item is n units, that it costs $\$k$ to place an order for this item over and above the cost of the goods ordered, and that the carrying costs are $\$c$ per unit per week. Suppose that reorders are placed in time so that the new stock arrives just when the old stock is exhausted. (The reader can see the difficulties hidden in this assumption when delivery lags are at all appreciable.) Then the only decision to be made is the quantity to order when an order is placed. Call it x. On these assumptions, n/x orders will be placed per week, on the average, giving rise to an average weekly order cost of $\$kn/x$. The size of the inventory will range from x units, just after new stock has been received, down to zero units just before replenishment, averaging at $\frac{1}{2}x$ units. The associated carrying cost is then $\$\frac{1}{2}cx$ per week and the total weekly cost of maintaining the item in inventory is $\$kn/x + \$\frac{1}{2}cx$. The optimal value of x is the one that minimizes this cost, and is easily found to be

$$x = \sqrt{2\,\frac{kn}{c}}\,.$$

Thus the optimal size of order and of average inventory, in these circumstances, varies in proportion to the square root of (*i*) the rate of consumption or sales, and (*ii*) the ratio of ordering cost to carrying cost.

The reader is surely unpleasantly aware of how many simplifying assumptions were made, implicitly and explicitly, in the course of this derivation. Most of them can be dispensed with, at the cost of increasing the complexity of both the argument and the result. Schlaifer [34, Ch. 15], for example, presents a full discussion of a case in which demand during the interval between placement and receipt of an order is a random variable, so that there is substantial danger of being out of stock before the replenishments are received. The general approach, the

calculation of average results, is the same but the technical difficulty of the analysis is magnified manyfold.

An essential prerequisite for using static analysis is that it be possible to divide up time into a sequence of independent periods. These periods need not all be of the same length or even of predictable length, but they must be independent in the sense that no decision or event in any period affects the situation in any subsequent period. In the preceding analysis, for example, we can define a period as extending from one date at which the stock is precisely x, the order quantity, to the next date at which this is the stock. Then periods will be of varying length, depending on fluctuations in rate of consumption, but at the outset of each period the position of the firm will be precisely the same as at the outset of any other period and the consequences of any inventory policy can be synopsized by taking the average results of that policy in a single period. That is just what we did.

Unfortunately it is not always possible to divide time up into such independent periods. For this there are several reasons. It may not be possible to order new stock just when the level of the inventory makes it desirable. Thus, canneries must contract for produce once a year, ships can replenish only when in port, etc. Sometimes even when irregular ordering is technically possible it may be undesirable. It may be administratively necessary to establish a periodic ordering routine, or it may be economical to consolidate orders for a variety of items into a single requisition. In such cases a schedule of ordering dates has to be established, and they become the natural points of division between time periods because they are the dates on which decisions have to be made. When a regular ordering cycle is established the periods cannot be independent because each period's initial stock is influenced by what went on before.

But independence may fail even when irregular ordering is permitted.[11] For example, if stock is exhausted orders may be backlogged. Then when new stock is received its utilization does not begin *ab initio* but is influenced by the extent of the backlog. Thus in many actual situations inventory policies have to be devised without making the assumption that time can be sliced into a sequence of independent periods. Then it is invalid to appraise an inventory policy by its average results in a single period; more complicated techniques have to be used.

Stationary-state analyses comprise one family of such techniques. If we contemplate a long sequence of periods extending into the future we can consider the probability distribution of the state of the firm (e.g., the size of inventories and backlogs) at the outset of each period. In the first period, of course, the initial state is known. The probability

[11] Howard Raiffa taught me this.

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distribution of the initial state of the second period depends on the initial state of the first period, the inventory policy followed, the probability distribution of demand (which we shall for simplicity assume to be the same in all periods) etc. The third period probability distribution depends on the second period probability distribution, the inventory policy, the probability distribution of demand, etc.[12] If we continue in this manner it fortunately happens that the influence of the initial conditions of the first period gradually dies out and the probability distribution of initial states generally ceases to change from period to period. In other words, a stationary state is attained.

Once the stationary state has been reached we can apply the averaging procedure, in two steps. First we consider each possible initial state in turn and compute the average results of the inventory policy in a period that begins with that initial state. Then we average those results over all possible initial states, giving each a weight equal to its probability. The result is an over-all average profit (or cost, if that is desired) per period in the long run.

TABLE 1—PROBABILITY DISTRIBUTION OF MONTHLY DEMAND FOR A PART

Number of times demanded	0	1	2	3
Probability	.5	.3	.1	.1

All of this sounds very complicated, and is, but a simple example may clarify it. Consider the problem of a machine shop that stocks a part that is expensive to store and is infrequently demanded. When it is demanded the shop gets a job if the item is in stock and nets, let us say $15; but if it is out of stock the sale is lost. The carrying cost for the part we take as $3.00 a month. Inventory is taken on the first month and if an order is placed the new stock is received on the first of the following month in time to be included in that month's inventory. The probability distribution of demand is given in Table 1.

With these data the consequences of any inventory policy, in the stationary state, can be computed. Three possible inventory policies are listed in Table 2. We shall consider policy I in detail, it being the least tedious. First note that if this policy is followed there can never be more than one part in stock. Next write down a table of "transition probabilities." These give the probabilities of either possible inventory level, 0 or 1, at an inventory date when the inventory level at the previous date is given. These are displayed in Table 3.

[12] It may also depend directly on events in the first period, as when delivery lags are longer than a period or when usage in any period affects the probability distribution of demand in several subsequent periods. We have ample precedent for neglecting this kind of complication.

TABLE 2—THREE POSSIBLE INVENTORY POLICIES; NUMBER OF PARTS TO ORDER WHEN CURRENT INVENTORY IS GIVEN

Current Inventory	Number of Parts to Order Under		
	Policy I	Policy II	Policy III
0	1	1	2
1	0	1	1
2	0	0	0
3 or more	0	0	0

This table states that if inventory is zero on one inventory date it will surely be one on the next (since one part will be ordered and none will be sold), and that if inventory is one on any date then the chances are 50-50 that it will be zero or one on the next date. Now let P_0 and P_1 denote the probabilities of the two inventory levels and assume that the stationary state has been reached, so these probabilities are constant over time. Then, from the first column of Table 3, the probability of having no items in stock on any date is half the probability of having one in stock on the previous date, or $P_0 = .5P_1$. From column 2, the probability of having one item in stock is the sum of two contingencies: none in stock on the previous date plus one in stock on the previous date and still unsold. Thus $P_1 = P_0 + .5P_1$. This, of course, is the same equation as before. But the two probabilities have to add up to unity. Thus $P_0 = 1/3$, $P_1 = 2/3$ are the steady-state probabilities of the two possible inventory levels.

If inventory is zero, there are neither sales nor inventory costs. If inventory is one, inventory costs of \$3 are incurred and there is a 50 per cent chance of making a sale, so that average profit is $\frac{1}{2}\$15 - \$3 = \$4.50$. The long-run average profit per period resulting from this policy is then $\frac{1}{3}(0) + \frac{2}{3}\$4.50 = \$3.00$. The other two policies in Table 2 can be assessed similarly. For policy II the steady state probabilities are 1/11, 5/11, 5/11 for stocks of 0, 1, 2 respectively, with an average profit per period of \$4.09. For policy III the probabilities are 1/9, 3/9, 5/9 and the average profit is \$4.00. Thus policy II is the best of the three.

TABLE 3—TRANSITION PROBABILITIES FOR POLICY I

Previous Inventory	Current Inventory	
	0	1
0	0	1
1	.5	.5

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This brings up three comments. First, the policy is a bit peculiar. It tells us that when stocks are down to zero we should order one part and then, whether or not it is still unsold at the end of the month, order another. Why not order the two together? Because inventory carrying costs are too high to justify carrying a second item in stock when there is sure to be a first item, but are not too high to risk carrying a second item when there is only a 50 per cent chance of having a first. Second, there is no unique optimal stock level in this example. If the part is out of stock we should order up to a stock level of one; if the stock level is one we should order up to a stock level of two. Now, much of inventory theory presumes that there is an optimal stock level and attempts to find it. In this case such a search would not discover the optimal available inventory policy.[13] Third, we have considered three policies and have found the best of the three, but how do we know that there is not a still better policy? Stationary-state analysis will not tell us, because the computations require the table of transition probabilities and these, in turn, require that the inventory policy be given. This mode of analysis is convenient for assessing the consequences of any given policy; it is inconvenient for discovering an optimal policy except in some special cases.[14] Thus we must advance to full-fledged dynamic analysis, which meets this need.

Dynamic inventory analysis rests on two ideas, both most thoroughly expounded by Bellman [7]. The first is a recurrence relationship connecting optimal inventory policies for different planning horizons. Suppose that on a given inventory date the stock is N and it is desired to take into account the consequences of any decision for H periods into the future. (H may be infinite but it will do no harm to talk as if it were finite.) Since we plan to take account of consequences that may extend over a considerable length of time, time preference becomes relevant. Therefore, assume that the current valuation placed on a dollar t periods in the future is $\$a^t$, i.e., assume a rate of discount of $(1\text{-}a)/a$ per period.

Suppose it is decided to order x units. The consequences of this decision can be divided into two parts: its effects on the present period and its effects on the $H-1$ subsequent periods. The effects on the current period are the average level of profits in a period with initial inventory N if x units are ordered. This value can be ascertained by the kind of calculation we have already illustrated. Denote it by $r(N, x)$. The effect on later periods is a bit more complicated. One cause of com-

[13] Dvoretzky, Keifer and Wolfowitz [16] present a full discussion of the conditions in which an optimal stock level exists.

[14] Situations in which an optimal stock level exists are among such special cases. Morse [31, Ch. 10] discusses the use of stationary-state analysis for finding optimal stock levels.

plication is that we do not know what the initial inventory of the second period will be. We can, however, calculate its probability distribution from the probability distributions of demand and deliveries in the first period. Accordingly, let $p_M(x)$ be the probability that the inventory inherited from the first period will be M if x units are ordered in the first period. We now assume, and this assumption will be justified later, that we already know the optimal inventory policy for an $H-1$ period horizon. That is, we know the maximum discounted profit that can be obtained in $H-1$ periods beginning with an inventory of M. Denote this number by $f^*_{H-1}(M)$. Then, taking the discount factor into account, the total present value of profit in H periods beginning with an inventory of N and ordering x units is $r(N, x)$ plus the average $H-1$ period profit taken over all values of M, or:

$$f_H(N, x) = r(N, x) + a \sum_M p_M(x) f^*_{H-1}(M).$$

Let x^* be the value of x that maximizes this expression. Then the maximum value of $f_H(N, x)$ is $f_H(N, x^*)$, and this is $f^*_H(N)$ in the notation previously introduced. We thus have the recurrence formula:

$$f^*_H(N) = \max_x \left\{ r(N, x) + a \sum_M p_M(x) f^*_{H-1}(M) \right\}$$

where all the numbers inside the braces are known. Thus we can find the value $f^*_H(N)$ and the corresponding optimal order quantity, simply by trying all permissible values of x and noting the one that maximizes the value in the braces. This may be laborious but it is a practicable undertaking by hand for small problems and by electronic computer for large ones.

The second basic idea of dynamic inventory analysis is that if we determine optimal inventory policies successively for planning horizons of 1, 2, 3, $\cdots$, periods, the sequence of policies will converge to the long-run optimum.

To see how this approach works let us return to the machine-shop example. For a planning horizon of one period $f_1(N, x) = r(N, x)$. But in this example, the placement of an order does not affect the current period, i.e., $r(N, x)$ is a constant in x. Let us then take the optimal

TABLE 4—$f_1^*(N)$ OR $r(N, O)$ FOR THE MACHINE-SHOP EXAMPLE

N	$f_1^*(N)$	N	$f_1^*(N)$
0	\$0	4	\$0
1	4.50	5	−3.00
2	4.50	6	−6.00
3	3.00	etc.	

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value of x to be zero, whatever N may be. Thus $f_1^*(N) = r(N, 0)$. Table 4 is a table of this function computed from the previous data on costs and the probability distribution of demand.

Next consider a planning horizon of two periods, using a discount factor of $a = .99$. Applying the basic formula for the consequences of ordering x units when N are on hand,

$$f_2(N, x) = r(N, x) + .99 \sum_M p_M(x) f_1^*(M).$$

For every possible value of N we now require the value of x that makes this expression as large as possible. The ensemble of these values of x will be the optimal inventory policy for the two-period case. The calculation is protracted, so we illustrate it for a single value, $N = 1$. In this case there is a probability of .5 that the part on hand will be sold. Thus $r(N, x) = \$4.50$ for all values of x and $M = x$ with probability .5 and $M = x + 1$ with probability .5. Substituting these results in the formula:

$$f_2(1, x) = \$4.50 + .99[\tfrac{1}{2} f_1^*(x) + \tfrac{1}{2} f_1^*(x + 1)].$$

The values of $f_1^*(x)$ and $f_1^*(x+1)$ for all values of x can be read off from Table 4. Thus we find that $f_2(1, x)$ assumes its maximum value when $x = 1$, so that

$$f_2^*(1) = f_2(1, 1) = \$4.50 + .99(\tfrac{1}{2}\,\$4.50 + \tfrac{1}{2}\,\$4.50) = \$8.955.$$

The calculation for other values of N is similar. Then, having found an optimal inventory policy and its results for $H = 2$ we advance to $H = 3$, which can be analyzed using the results for $H = 2$ and the fundamental recurrence formula. After that we go on to $H = 4$, 5, etc. Table 5 summarizes the results of a number of these computations which, remember, are readily mechanized. The entries in this table, of which we computed the one for $N = 1$, $H = 2$, are the optimal size of order for the given initial stock and planning horizon. Note that in this instance the optimal policy is the same for all horizons of three months

TABLE 5—OPTIMAL INVENTORY POLICIES FOR A NUMBER OF INITIAL INVENTORIES AND PLANNING HORIZONS

(Number of parts to order with given initial stock)

Initial Inventory	Planning Horizon in Months				
	1	2	3	4	5
0	0	1 or 2	1	1	1
1	0	1	1	1	1
2	0	1	0	0	0
3	0	0	0	0	0
4	0	0	0	0	0
5	0	0	0	0	0

and greater. The convergence of the short-horizon optimal policies to the long-run optimum is not always this fast, of course.

As this illustration shows, dynamic inventory analysis leads straightforwardly to the discovery of the optimal inventory policy for given data. Its major drawback, aside from its laboriousness, is that it does not lend itself to analysis. It just gives the answer without disclosing how the optimal policy would change in response to changes in any of the data. If any of the data (such as prices, probability distribution of demand, delivery lag, etc.) should change there is nothing to do but to perform the whole computation over.[15]

II. Ad Hoc *Models, Simulation*

1. *Two More Models: Queuing and Sequencing*

Limitations of space and time fortunately prevent me from extending this catalogue of methods used in operations research. There are, however, two more important families of models that must at least be mentioned: queuing theory models and combinatorial models.

Queuing problems occur very widely: the unanswered letters in your desk are a queue, so are the aircraft waiting to land at a busy airport and the machines standing idle waiting for the repairman. In general, any backlog of tasks waiting to be performed is a queue. (The tasks may be very simple, like admitting passengers through a bank of turnstiles, or they may be complicated, like treating casualties after a battle.) Queues are generally unpleasant to the people who are waiting for the service, and frequently expensive. On the other hand the provision of additional or improved serving facilities to reduce or eliminate the queue is also expensive. The task of queuing theory is to determine the optimal quantity and characteristics of the facilities that serve a queue, having regard to both of these costs. It performs this task by studying the way in which the serving facilities influence the probability distribution of the length of the queue, the probability distribution of waiting times, etc., treating the arrivals to the queue as a given datum.

Queuing problems are closely related to inventory problems. Both models are concerned with accumulations (of tasks or of stocks as the case may be), with accretions to them and with subtractions from them. In inventory problems the accretions are subject to at least partial control, through reordering policy, but the subtractions are governed by a random process that is largely beyond the control of the firm or organization that maintains the inventories. In queuing problems the

[15] Exceptions to this dictum occur when the probability distribution of demand and other data are sufficiently tractable. A few instances are given in Arrow, Karlin and Scarf [4].

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reverse is true: the accretions are random and beyond direct influence while the subtractions can be controlled through control of the serving facilities. Thus it is not surprising that the analysis of queues depends on the same general principles and methods as the analysis of inventories though, of course, there are substantial differences in detail.

The most interesting general conclusion yielded by queuing theory is the following: Consider a queue attended by a single server. Suppose that tasks arrive, on the average, one in every a minutes and require, on the average, b minutes to perform. Then in a fairly long interval, of length T, say, approximately T/a tasks will arrive, the server will be busy Tb/a minutes, and the average proportion of the time that the server will be busy will be b/a. This ratio, known as the utilization factor, is the key to the behavior of the queue. If it exceeds one, and the system doesn't break down, then obviously the length of the queue will grow towards infinity and so will the average waiting time. It is a little shocking to learn that this same result holds if the utilization factor equals unity, but this fact seems more reasonable when we recall that the queue is caused by the fact that the number of arrivals in any time interval is a random variable and the variance of this variable increases indefinitely as the length of the interval increases. Finally, if the utilization factor is close to unity, the average waiting time will be finite but large. It follows that if the queue is to be kept short, the utilization factor must be substantially less than unity, i.e. the server must be idle a large proportion of the time. Thus, ostensibly idle capacity is essential to prompt service. Old-fashioned efficiency experts will please take notice.

Combinatorial or sequencing models are used most frequently to describe production problems in which the decision to be made concerns the order in which certain operations are to be performed. Typical applications are decisions as to the order in which the parts of a complicated product are to be assembled (e.g., it is obvious that you should put on your socks before your shoes, but less obvious whether the socks should precede the trousers) and, in a job-shop, the order in which jobs should be performed (e.g., if black and white paint are to be mixed on the same machine it is usually better to mix the white paint first). Another group of combinatorial problems concerns the assignment of tasks. Thus, on a moving production line the various operations should be assigned to the various stations so that (a) operations are not performed in an awkward order, and (b) the operations assigned to all stations should require, as nearly as possible, the same length of time. The hallmark of a combinatorial problem is that it cannot be formulated as the choice of a value of a quantitative variable. Thus the most powerful tools of mathematical analysis, algebra and the calculus, do

not apply and problems that appear quite innocent on the surface are likely to prove very intractable. Nevertheless combinatorial problems can frequently be solved, sometimes, surprisingly, by an adaptation of linear programming.

2. Ad Hoc *Models*

I am fighting against giving the impression that this list of ready-made models, or any similar list, can include the bulk of the conceptual frameworks used in operations research. On the contrary, most operations research work employs *ad hoc* models, exemplified by the one in the soap advertising fable. In order to redress somewhat the balance of emphasis I must therefore sketch, very cursorily, two illustrations of *ad hoc* approaches, both taken from the very valuable compendia, *Operations Research for Management* [26] [27].[16]

The first illustration concerns the efficient utilization of the vehicular tunnels operated by the Port of New York Authority, one of the prominent users of operations research [33]. The problem was to determine the traffic conditions conducive to the largest feasible flow of traffic through the tunnels, in vehicles per hour. The volume of traffic, V, in vehicles per hour, is the product of traffic density, D (vehicles per mile), and speed, S, in miles per hour, as can be seen by noting that if traffic moves at S miles per hour all the vehicles within S miles of a given point will pass that point in an hour. Speed and density are closely related. It was assumed on the basis of previous empirical observations that the relationship was linear and, indeed, traffic counts in the fast lane of the Lincoln Tunnel led to the regression equation $S = 42.1 - .324D$ with a correlation coefficient of $r = .97$. Multiplying both sides of this equation by D therefore yields $V = DS = 42.1D - .324D^2$. Differentiating this expression with respect to D and setting the derivative equal to zero produces an optimal density of $D = 65$ vehicles per mile, corresponding to a speed of 21 miles an hour and a vehicular spacing of 81 feet. How to persuade drivers to adopt that speed and spacing is another problem, not dealt with in this report. A minimum speed of 20 miles an hour was posted and had a discernible effect in increasing the flow of traffic.

This project, of course, employed a very simple model, invoking only the empirical functional relationship between speed and density and the definitional relationship of those two variables to volume. The bulk of the work in this case was observational and statistical: observing and analyzing traffic conditions in the different tunnels and lanes. But the model, primitive though it was, was at the center of the project, dictating what was to be observed and how the results were to be used.

[16] Summarized with the kind permission of the publishers, The Johns Hopkins Press.

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My second example is inherently more tedious to describe because I have selected it in order to illustrate the kind of elaborate technical and technological analysis that is a part of the work of operations research. I shall therefore, in the interest of brevity, do considerable violence to the actual facts of life. The reader who wishes a more accurate description of the project is referred to the original report by Dunlap and Jacobs [15].

The project dealt with strip mining of phosphate rock. This is accomplished by enormous power shovels called draglines, costing over $7 million each, with buckets of as large as 30 cubic yards capacity, and earth-moving capabilities of as much as 1,500 tons an hour. In operation they excavate a linear strip of ore that can be several hundred feet wide by taking a certain stand, excavating the ore that can be reached conveniently from that stand, then backing up a distance along the strip, excavating again, and so on. The shape of the hole dug at each stand may be visualized as a piece of pie with the point cut off or as a segment of a doughnut. But, and now we come to the problem, both the geometrical shape and the dimensions of the excavation made at each stand are variable within very wide ranges. The problem was to ascertain the optimal shapes and dimensions to employ, depending on the width of the ore vein, the thickness of the ore vein, and its depth below the surface. More specifically, the problem was to find the mode of operation of the dragline that would maximize the tonnage excavated per hour.

The opposing considerations, severely simplified, were as follows. On the one hand, a dragline is an unwieldy vehicle, to say the least. It moves slowly, cannot dig while it is being moved, and what is more to the point, the time required to prepare it for moving and to unlimber it after each move is considerable. Thus it is desirable to move it as infrequently as possible. On the other hand, as we shall soon see, the rate of excavation per hour is adversely affected if the area excavated at each stand is too large. The analysis of this second effect was the heart of the problem. To introduce the quantitative concepts let V denote the volume excavated at a single stand. Then, as you may recall from elementary calculus, if the shape of the excavation is a segment of a doughnut

$$V = \int_{r_1}^{r_2} \int_{\theta_1}^{\theta_2} \int_{z_1}^{z_2} r \cdot dz \cdot d\theta \cdot dr.$$

In this formula r denotes the distance from the dragline cab to an elementary volume of ore and varies from r_1, the radius of the inner rim of the doughnut, to r_2, the radius of the outer rim. θ indicates the angle from the cab to the elementary volume of ore, measured from the center line of the strip being excavated. It varies from θ_1, the maximum

angle of swing to the right, to θ_2, the maximum swing to the left. Finally, z is the depth below ground level of the elementary volume of ore, varying from z_1, the depth of the top of the vein, to z_2, the depth of its bottom. Of these variables, z_1, z_2 and the width of the vein are determined by geological happenstance while r_1, r_2, θ_1, θ_2 are subject to decision within wide ranges. The problem is thus to determine optimal values of the last four variables and, moreover, since it is not necessary to cut in doughnut shape, the entire geometric configuration of the excavation. This last, nonquantifiable, range of possibilities helps make the problem really fascinating, but we shall for simplicity continue to pretend that the excavation will be doughnut-shaped.

We now introduce the considerations that determine the time required to excavate a volume V from a given stand. The elementary cycle of operation begins when a load has just been discharged at the dumping point on the rim of the excavation. It consists in swinging the bucket back to the elementary volume of ore to be excavated, filling the bucket, swinging back to the dumping point, and emptying the bucket. The time required for the filling and emptying operations does not depend on the location of the unit volume of ore being excavated; we denote it by t_d. The time required for swinging to and from the point of excavation does depend on where the point is. We denote it by the function $t(r, z, \beta-\theta)$, where β is the angle from the dragline to the dumping point and all other variables have been defined. Finally, the number of times that the bucket must be returned to the point (r, z, θ) is the ratio of the elementary volume at that point, $r \cdot dz \cdot d\theta \cdot dr$ to the bucket capacity, denoted by D. Assembling these expressions, the total time required to excavate volume V is:

$$T = \frac{1}{D}\int_{r_1}^{r_2}\int_{\theta_1}^{\theta_2}\int_{z_1}^{z_2}[t_d + 2t(r, z, \beta - \theta)]r \cdot dr \cdot d\theta \cdot dz.$$

This formula gives the time actually spent in excavating the volume V. In addition, in order to obtain this volume the dragline must be moved into position, which means that it must be shut down after finishing at its previous stand, moved, and unlimbered. The time required for actual moving is irrelevant since the dragline will have to travel the entire length of the strip no matter what the mode of operation, but the time consumed in shutting down and setting up has to be charged against the V cubic feet excavated at the stand. Denoting this time by t_p, the total time required to obtain the V cubic feet at a stand is $t_p + T$ and the volume excavated per hour is $V/(t_p + T)$. This is the figure of merit, the quantity to be maximized by proper choice of operating parameters. If V and T are small, the critical ratio will be small because of t_p in the denominator. If V and T are large, the ratio is approximately V/T, and may be small because of the time con-

sumed in swinging the bucket back and forth over large angles. It appears that there is likely to be some intermediate optimum.

Clearly this formulation of the problem, the identification of relevant variables, the rejection of minor variables, and the determination of how the relevant variables affected the results, was attained only after careful observation of the actual operation and hard, careful thinking. There was additional empirical labor, also. The formula, it will be remembered, involved the swinging-time function, $t(r, z, \beta - \theta)$. This function had to be determined statistically by observing a great many excavations with different operators and working conditions. Other relationships, not mentioned in my summary, also had to be estimated empirically.

Even after the formula relating cubic yards per hour to the operating parameters had been established, substantial difficulties remained. As I set it up, there were 4 parameters to be decided; in the actual problem there were 10. Now, finding the maximum of a complicated function with respect to 8 or 10 decision parameters is a formidable undertaking. (This contrasts with the vehicular tunnel example, where the maximizing step was trivial.) The report does not state how the optimal decision parameters for different geological conditions were determined, other than that "it was necessary to carry out the total set of solutions on a high-speed computer"[17] [15, p. 191].

And still, when the optimal modes of operation had been determined, the task was not completed. The recommendations had to be applied by foremen who are not skilled in substituting in mathematical equations or even in using ten-dimensional tables. Thus the recommendations had to be translated into a number of usable guide-charts and nomograms together with an extensive manual of instructions. One of the analysts spent three months with the operation after the conclusion of the study, training the personnel in the application of the results. All this effort seems to have been worth while since it led to an increase of some 40 per cent in output per hour.

3. *Simulation and Gaming*

This extended example may suggest the extreme mathematical, statistical, and technical difficulties that confront the operations analyst. They occur whether special-purpose or general-purpose models are employed. I have already mentioned that in the area of general systems analysis the equations describing the performance of an organization defy solution more often than not. The same is true of inventory problems and queuing problems. Even linear programming, whose salient

[17] This kind of vagueness, enforced by the proprietary nature of many of the data, mars much of the literature of operations research.

virtue is the ease with which it lends itself to solution, is constantly pressing against the limitations of the most modern and powerful computing machines.

As a result, the operations analyst, like every other research worker, lives nearly always near the end of his tether. He simplifies his problems as much as he dares (sometimes more than he should dare), applies the most powerful analytic tools at his command and, with luck, just squeaks through. But what if all established methods fail, either because the problem cannot be forced into one of the standard types or because, after all acceptable simplifications, it is still so large or complicated that the equations describing it cannot be solved? When he finds himself in this fix, the operations analyst falls back on "simulation" or "gaming."

Simulation is to the operations analyst what a pilot model or experiment is to a natural scientist. If you can't predict how a machine will behave the only thing to do is to try it out and see. The operations analyst cannot usually try out an enterprise of the characteristics he is considering, but he can frequently duplicate it, at least approximately, on paper. To see how this works, pretend that we had failed to solve the machine-shop inventory problem of the last section. Then we should have to analyze it by simulation.

The most popular methods of simulation use high-speed computing machines. To simulate our inventory problem we should select a starting inventory at random, read the data of the problem into the machine and instruct the machine to follow some specific inventory policy. The machine would then look at the given starting inventory and decide in accordance with the assigned inventory policy whether to place an order for replenishment and if so how large. Then it would draw a random number from the range 0 to 9 inclusive. If the number were in the range 0-4 it would interpret this to mean that no parts were demanded in the first month, if it were in the range 5-7 it would assume that one part was demanded, an 8 would mean that two parts were demanded, and a 9 would represent a demand for three parts. Whatever the result, the machine would satisfy the demand as far as possible, subtract those sales from the initial inventory, increase the inventory by the replenishment stocks received in response to orders placed, if any, print out the results of interest, and go on to perform the same calculations for the second month.

All this would take about a thousandth of a second.[18] In this way the machine would generate a thousand months of synthetic experience with the assigned policy, equivalent to nearly a century, in a second.

[18] So much celerity is, unfortunately, extremely rare. Half a minute to a minute of machine time (costing $5 to $10) per cycle would be more typical.

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When a sufficient amount of experience with a given inventory policy had been accumulated the machine would calculate the average inventory, average number of sales per month, average number of refusals, average reorder and inventory carrying cost, etc. Then it would turn to a new inventory policy and perform the same calculations.

In this way very large samples of synthetic experience can be obtained quickly and estimates can be obtained of all desired characteristics of probability distributions that are too complicated to be calculated mathematically. Analyses by simulation can do even more than that. The machine can be programmed so that after it has tried a few inventory policies assigned by the analyst it will then review the results and decide which would be the most promising inventory policy to try next. Then it could try the policy it has selected, again review the results, concoct a new promising policy, try it, and continue this process of trial and revision until it could not find any avenue that promised improvement within the range of inventory policies that it was permitted to explore. All this a calculating machine can do quickly, accurately, and without human intervention. What more could be desired?

Well, unfortunately, a great deal more. The result of a simulation is always the answer to a specific numerical problem without any insight into why that is the answer or how the answer would be influenced by a change in any of the data. In our inventory example, a change in the probability distribution of demand, in the length of the delivery lag, in the reorder cost or in net profit per sale would presumably change the solution, but a simulation with given data gives no hint of size or direction of changes in inventory policy resulting from such changes in data. Thus each variant of a problem analyzed by simulation has to be solved by a separate computation; and computation is expensive. In practical affairs, of course, it is usually more important to know how to respond to changes in conditions than how to behave optimally in any single set of circumstances. This is so because, first, circumstances do change, and second, because we never do know precisely what circumstances are but have to base decisions on estimates and, therefore, have to know how consequential errors in these estimates are.

There is a second serious limitation, also. Above I said that a computing machine could be programmed to search iteratively through a family of possible inventory policies and, through a guided process of trial and error, pick out the best. This is an oversimplification if the problem is at all complicated, say complicated enough to warrant simulation. Most decision problems tackled by operations research involve a number of interrelated variables. In the inventory example the variables are the numbers of parts to be ordered when the inventory is at each of its possible levels. In transportation problems the variables are

the quantities to be delivered by each supply point to each consumer. A review of the other models we have discussed will show that typically they are multidimensional and that much of their difficulty stems from the wide variety of possible solutions to be contemplated. When explicit methods of solution cannot be found, therefore, we find ourselves in an area known as "analysis of response surfaces," about which a few words have to be said.

Suppose that we are interested in maximizing profit or minimizing cost, or something else of the sort, where the profit or cost in question depends on a number of variables under our control but where the manner of dependence is so complicated that we cannot actually write it down in the form of an equation. Then we can select any set of values of the variables under our control and, say, by simulation, estimate the value of the profit corresponding to that selection. This profit, together with the values of the variables that gave rise to it is "a point on the response surface," and, subject only to limitations of patience and expense, we can calculate any number of such points that we please. What we now need is some procedure for finding the set of values of the decidable variables that gives rise to the highest possible profit, by means of a practicable number of simulations or similar computations. All that simulation provides is a method for finding single points on the response surface and the best that can be said about finding optimal points is that research on this problem is being prosecuted vigorously [9] [10]. As things stand at present no fully satisfactory general-purpose method is known.

Be that as it may, simulation comes nearer to solving the unsolvable than any other device known, a fact that justifies fully its importance as a tool of operations research. Except in problems as trivial as our inventory example it is, however, a difficult tool to use well. It entails two main kinds of technical difficulties, those relating to the exploration of the response surface, which we have already discussed, and statistical problems such as deciding how large a sample to take of each set of circumstances and policies. E.g., in our inventory example the statistical problem is how many months of synthetic experience should be accumulated with each inventory policy examined. With a given research appropriation, more policies can be examined if each examination employs a smaller sample, but the disadvantages of using unduly small samples is evident. These are formidable problems in technical mathematical analysis and statistics and have an important bearing on the cost and precision of the analysis, but do not have much influence on the result, or, at least, should not.

A device quite similar to simulation in form but entirely different in objective is "gaming" or "operational gaming." Formally, gaming is

simulation with human intervention. To "game" our inventory example we should omit the inventory policy from the machine program and replace it by the following routine. Each time that a new initial inventory is computed, the machine would print out the results of the previous period's operations, including the terminal inventory, and wait. Then the subject, which might be either an individual or a team, would decide on the basis of his best judgment how many parts to order. This would be read into the machine which would then compute the results of the period's operations, print them out, and wait for the next decision. In working through an operational game of this sort, the subject might or might not be informed of the basic data, for example the probability distribution of demand. If he is not informed of some of the relevant data he would be expected to deduce them as experience accumulates.

This device will not, of course, disclose the optimal policy to be followed, but it can serve any of several other purposes. It can be used to investigate how close an experienced subject can come to the mathematical optimum in given circumstances and how long it takes him to do so. It can be used to test how changes in the circumstances and in the data available change the performance of subjects, and in this way to throw light on the practical value of additional information in the real life situation being simulated. Thus, in the inventory example, if the probability distribution of demand is not known such an experiment could reveal how much should be spent on market research or other methods for ascertaining it.

Gaming can also be used as a psychological-experimental device for studying executive behavior. Thus, in one set of such experiments it was found that executives started by basing their decisions on rough rules of thumb and revised them, in the light of experience, much too slowly in the sense that when they changed their policies they usually moved them only a small fraction of the distance between the current policy and the optimum, and only very rarely overcorrected. Further it was found that this conservative bias tended to be more marked in proportion to the importance of random and unpredictable elements in the game.

Gaming can also be used to study the optimum of some decisions when other decisions are too complicated or are based on considerations too vague to be formalized. In this application, various policies for making the decisions to be optimized are programmed into the machines while the unmanageable decisions are made by a team as required. Finally, gaming can be used as a pedagogical device.

Gaming is fun but, if a skilled team is required, very expensive. The expense frequently precludes sufficient replication to generate reliable

probability distributions of consequences; and even where expense is not prohibitive, the memories, learning processes, and tendencies to habit formation on the part of the teams make much replication impracticable. Even if the replication problem can be surmounted there is a more fundamental difficulty in using gaming as a research tool. In any series of repetitions of a game, the teams will either base their decisions on some well-defined policy or, more usually, will "play it by ear," using their best judgment as experience accumulates. In the former instance the results of the experiment will be an assessment of the consequences of the policy used but, as we have seen above, any well-defined policy can be assessed more cheaply and conveniently by programming a computing machine to follow that policy and conducting a simulation experiment. In the latter instance it is hard to say what the results mean since no expressable or reproducible policy was followed. The results will reflect an amalgam of the potentialities of the situation, the approximations used in constructing the game, the abilities of the teams under somewhat unnatural conditions, and the vagaries of chance. If a large number of teams is used, the gaming may produce an evaluation of how well an "average" team will perform under the conditions of the game, but still another dimension of sampling variability crops up in this application [38].

As a result of these problems, the literature does not indicate that gaming has played a significant role in solving operations research problems. It seems to hold more promise as a device for executive training, for investigating the psychology of decision making, and for stimulating effective planning by confronting managers vividly with various contingencies they should be prepared to meet. Even in these last applications, however, it does not seem feasible to impose rewards and penalties cruel enough to approximate the pressures of real-life decision problems.

III. *The Objective Function*

A review of the models we have considered will show that each can be divided into two parts: a part describing the structure of the operation and the relationships among the variables (both controllable and uncontrollable), and a part that evaluates the consequences of any choice of variables in terms of profit, cost, or some other measure of desirability. We shall refer to the first part as the constraints and to the second as the objective function or criterion. Most operations research problems take the form of searching for the values of the decidable variables that maximize or minimize the value of the objective function while satisfying the constraints.

An economist can sympathize readily with this habit of formulating

problems as if the purpose were to maximize this or minimize that, but he is also aware of the pitfalls in this approach. In the first place, it is by no means axiomatic that the purpose of an operation can be expressed as maximizing anything less vacuous than desirability all things considered. The objectives of business enterprise are obscure. Among recent economic writers, Baumol [5] has argued that businessmen typically seek to maximize sales volume subject to a constraint on the rate of profit, Lanzillotti [24] found that predominantly they seek to attain some target rate of profit, Simon [37] maintains that they are "satisficers" attempting to reach some aspiration-level but not trying to maximize anything in particular. It seems clear that short-run profit maximizing is neither a sensible nor a prevalent business objective but, beyond that, what objectives are prevalent is a matter for conjecture.

In any specific context, then, the operations analyst has the task of ascertaining his client's objectives, and this task is complicated by the fact that his client is not likely to be very clear on the matter himself nor, since the client is generally not a single person, entirely of one mind. An appealing way out is to ask the client what his objectives are. B. O. Koopman, in a thoughtful presidential address to the Operations Research Society of America [23], stigmatized this practice as "authorititis" and included it in his catalogue of prevalent fallacies in operations research. His point was that no businessman, still less a second vice-president in charge of planning and research, can be expected to answer such questions with enough precision or authority to provide a sound basis for research.

The question is far from academic because typically a business firm watches manifold consequences of its operations including rate of profit, value of its shares, sales volume, share of market, public image, reputation of product, *et hoc genus omne,* and is not willing to increase its performance in any of these respects at unlimited sacrifice in the others. What is needed for operations research is a reasonably precise idea of how the varied goals trade off against each other, but in fact the interrelations are extremely complex. It is a frequent experience in operations research, when the job is done and the recommendations are presented, to have them greeted with, "Oh, but I guess we forgot to tell you that in this company we are very anxious to keep our reputation for prompt deliveries by having a separate warehouse in every major market area, even if some of them run at a loss." This sends the analyst back to his drawing board. Even though it does endanger the analyst's rapport with his client, it might not be so bad if it caused only a revision or two in each project, but the number of controlling policies that amount to collateral objectives can be very large and so well understood within the organization that no one bothers to mention them until they

are elicited by deep probing or a final report that violates them. A case in point is our example applying linear programming to the scheduling of petroleum blending operations. Our conclusion was that the refinery should produce either regular or premium fuel, but not both. This recommendation would almost certainly be unacceptable since refineries are generally committed to producing a full line of products. Linear programming, which tends to yield extreme (all or nothing) solutions, is especially likely to recommend insufficient variety of products or action.

Typically where there are manifold objectives, as there nearly always are, some of them are treated within the company as constraints, or limits on the concentration of effort on the attainment of some of the others. In Baumol's experiences, for example, the rate of profit, which economists traditionally regard as a primary objective, tended to play the role of a constraint on efforts to maximize sales volume. How does one tell the difference between a constraint and an objective? In principle, I suppose, it cannot be done. John Dewey taught us long ago that means and ends, constraints and objectives, might roughly be separated for purposes of discussion but were so intimately intertwined that they were fundamentally indistinguishable.

Conceding this, a practicable distinction between constraints and objectives might go as follows: A requirement is a constraint if (a) it must not be violated at any cost however high or with any probability however low, and (b) there is no gain or advantage in overfulfilling it. On the other hand, a requirement is one of the objectives of the firm if it can be violated, though at a cost or penalty, or if there is an advantage in overfulfilling it. Mixed cases can occur. Thus, if deliveries must be made within two weeks at all cost, and if there is an advantage in quicker deliveries, the maximum delivery time is a constraint while, say, the average delivery time enters into the objective function. If this distinction be accepted, it will be seen that only technological requirements will qualify as constraints (e.g., the output of a refinery cannot exceed the volume of crude oil inputs); the attainment of all other requirements is part of the objective. In other words, every practicable design of an operation is subject to failure in sufficiently adverse circumstances; one designs so as to balance the risk of failure against the cost of decreasing that risk. Thus the use of policy constraints, though prevalent and perhaps inevitable, must entail some loss in attainment of the "real" (alas, inexpressible) purposes of the enterprise.

To get on with the discussion, suppose that it is possible to decide which of the consequences of an operation are constraints and which are objectives. The problem remains of combining the various objectives into a single objective function to be maximized. The difficulties

encountered here are familiar to economists for most part, but worth summarizing. There is, first of all, the problem of comparing consequences that occur at different times. The comparison of a number of courses of action whose consequences extend over time requires either the comparison of a number of detailed time paths, which is too difficult for most human minds, or some discounting procedure that accords relative values to costs, revenues, etc. at different dates.

The fact that the consequences of decisions are uncertain gives rise to a similar difficulty. The result of a decision or action is not, strictly, a predictable cost or income but a probability distribution of costs or incomes. Hence the comparison of the desirability of consequences presumes that we are able to compare the desirability of probability distributions. Still a third difficulty of the same general type arises from the conundrum that we have already discussed at some length, namely that not all of the consequences of a decision are commensurable in any convenient unit. A more specific example of this difficulty arises in inventory theory. If inventories are adequate or overadequate to meet demand, the consequences take the form of sales, carrying charges, and the like, all of which are easily measured in dollars. But if inventories are inadequate to meet demand, the consequences will lie in a different realm: there will be disruption of production processes, loss of customer's good-will, and so on. Ultimately these consequences too may be reflected in dollars and cents, but the measurement of such indirect monetary effects is a research project of forbidding difficulty. In queuing problems, again, the different consequences flowing from the same action tend to be incommensurable.

I have grouped together these three difficulties—time discounting, risk preference, and incommensurability of consequences—because they all have to be handled in about the same way. One conceptual approach is to try to construct preference maps. Fisher's theory of interest, for example, applies this construction to the time discount problem. A number of treatments of the risk problem, for instance Shackle's [35] and Markowitz's [29] are based upon it, and its relevance to the incommensurability problem is obvious. This device, however, is little more than a way of formulating the issue, because preference maps are almost impossible to ascertain empirically.

Another approach is to take the position that these complicated, multidimensional consequences are not the ultimate objectives served by the operation but are only intermediate ends to some ultimate, unidimensional goal such as maximizing the current net worth of the enterprise. This approach requires that we be able to measure the influence of each aspect of the intermediate consequences on the ultimate goal and, as suggested above, this task is generally prohibitively difficult.

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It amounts, of course, to attempting to construct the over-all utility measure that lies behind the preference map utilized in the first approach, and presumes that there is such a measure.

Still another approach is to apply a Paretan criterion as far as it goes and then to turn the task of further evaluation and choice back to the client. The analyst who follows this approach limits himself to seeking efficient decisions, i.e., decisions that cannot be improved in any dimension without an offsetting sacrifice in some other, and leaves it to the client to decide which of all efficient decisions he prefers. Of course, there are other devices, too, familiar to economists since the problems are familiar ones, which are more effective in eliminating grossly inappropriate decisions than in optimizing anything to a very fine degree. A typical one would be to work through the consequences of a decision, find that it would be optimal if the rate of time preference were, say, at least 15 per cent per annum, and then put it up to the client to decide whether his rate of time discounting is in fact that high.

The problem of evaluating risk preferences has received more attention lately than the other two problems because it lies at the heart of statistical decision theory. A number of principles for choosing among policies whose consequences are uncertain have been proposed, all plausible, none free of serious defects; but to discuss them would take us far afield. Illuminating surveys of thinking about decision-making under uncertainty can be found, for example, in Arrow [3] and Luce and Raiffa [25, Ch. 13].

Fortunately, this galaxy of unsolvable problems is less obtrusive in narrow operational contexts than in broad strategic ones. It is easier to ascertain the objectives of a department than of a firm, of a section than of a department, of a particular phase of the work than of a section. There are several reasons for this. First, the narrower the scope of an operation, the narrower is the range of consequences and the greater is the likelihood that all of them will be commensurable, usually in terms of dollars. Linear programming models are cases in point, though queuing and inventory models are contrary instances. Second, departmental and subdepartmental decisions frequently concern matters that have short time horizons; often all the consequences are practically immediate. Nearly all the models discussed above except the inventory models illustrate this assertion. Finally, the range of uncertainty associated with detailed operating decisions is generally smaller than that surrounding more comprehensive choices and, besides, the range of uncertainty engendered by each operating decision is small enough in relation to the size of the enterprise and such decisions are numerous enough so that they can be treated appropriately from an actuarial point of view.

For all these reasons the objectives of operations conducted at the middling and lower levels of an enterprise are likely to be fairly well defined. There is, however, one special danger that arises when an objective function is devised for a part of an organization or a specific operation. This is the danger that economies and diseconomies external to the department in question will be neglected. When an objective function is adopted for, say, a department, that department and the operations analysts who advise it will be induced to make the choices that seem optimal in the light of that function. Therefore it is important that such partial objective functions conduce to decisions that are consistent with the over-all goals of the enterprise. Unfortunately, it is just about impossible to find performance criteria that meet this requirement in all circumstances. A lurid example of this difficulty arose in wartime operations research in the air force, where the efficiency of bombing groups was judged in large part by the percentage of their bombs that fell within a thousand feet of the targets they aimed at. This standard encouraged bombing commanders to refrain from releasing bombs when visibility was poor. The effectiveness of the bombing groups went down while their paper scores went up.

The same conflict between real and paper performance occurs in business operations. Suppose, for example, that a manufacturing department is given the objective of producing its assigned output at minimum average cost. Then the manager being judged by this criterion has a strong incentive (a) to avoid overtime work even though other departments or customers may urgently require his output, and (b) to shave his specifications in a way that will increase the proportion of rejects at a later stage of fabrication. Any simple criterion of departmental performance must therefore be hedged by some artificial standards or constraints—like the subsidiary objectives discussed above—for example, quality standards and regulations about permissible delays in delivery. The shortcomings of such standards have already been mentioned.

This is probably as good a place as any to call attention to another pitfall in the selection of objective functions or performance criteria. This is the seductive simplicity of critical ratios like cost per unit, output per man-hour, or the one I used in the dragline example, cubic yards per operating hour.[19] Such ratios are invariably dangerous, as Hitch and McKean, particularly, emphasize [21]. Consider the dragline case. The criterion I suggested would discourage excavating to the very edge or bottom of the vein, even though it would be profitable to do so, since doing so would reduce the time-rate of excavation. There

[19] In fairness to Dunlap and his associates I must point out that a somewhat more sophisticated objective function was used in the actual study, though the published report did not define it precisely [15, p. 182].

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are two fallacies tucked away in my criterion. First, it assumes that dragline time is the only scarce resource, and encourages economizing it at the expense of all other resources, such as unexcavated ore. Second, it is a ratio and, as economists are well aware the objectives of an operation are almost invariably to maximize some difference (some measure of benefit minus some measure of cost) rather than any ratio. Profit maximization is not the same thing as unit cost minimization.

Thus there are dangers in assigning objectives to parts of an enterprise, but they are far more tractable than the problems encountered in trying to establish objectives for an enterprise as a whole. Because of the relative concreteness of the purposes of parts of an enterprise, suboptimization is a valuable concept and applicable even when over-all goals must remain vague. Suboptimization will be treated briefly in the final section.

IV. *The Role of Operations Research*

Such is the nature of operations research. I hope that I have made an adequate case for the assertion that its essence lies in a strong tendency to tackle administrative problems via formal models constructed in the spirit of the natural sciences. We turn now, and finally, to the role of operations research in business and economic administration.

If an experienced operations analyst were asked to describe the problem he would most like to meet, I suspect that he would mention four characteristics: first, the objective of the operation should be clearly defined, second, the operation should be describable by a reasonably manageable model, third, the data required for estimating the parameters of the model should be readily obtainable, and fourth, current operating practice should leave plenty of room for improvement. These are the characteristics of the ideal problem, from the analyst's point of view, and sometimes he encounters it, but more often he must be content with projects that fall short of perfection in one respect or another. Each of these characteristics deserves a little discussion.

For an operation to have a clearly defined objective it is not necessary, of course, that the businessman or other client be able to write down its objective function at a moment's notice. It does require that the analyst be able to obtain a consensus on the purpose of the operation, specific enough so that he can judge how conflicts in goals are to be resolved. With a little care to assure that the objective function does not conflict with higher-level goals and that the measure of cost is appropriate, the definition of objectives should present little difficulty at the operating levels. More trouble is likely to occur at the executive levels, where decisions are likely to have widespread and incommensurable ramifications. Glen Camp, in fact, warns against undertaking

such problems: ". . . best results will be obtained if the scientist meticulously avoids the evaluation of intangibles" [11, p. 630]. The narrower and more specific the problem, then, the more likely it is to possess this characteristic.

The second characteristic of a promising project was that it be possible to formulate a manageable model to describe the impact of various possible decisions on the objective function. Again we may cite Glen Camp: "The function of the operations research team is to assist the responsible authority of an organization by clarifying those uncertainties in the factors on which action is based, and *only* those, which can be clarified by scientific study" [11, p. 629]. Now, how is one to tell, in a preliminary survey of a problem, whether its essence can be caught in a manageable model or whether, in Camp's words, it can be clarified by scientific study?

Model building is the analyst's primary skill and contribution, and he cannot expect when he approaches a problem to find that his work has already been done for him. Thus the question is not whether a model exists ready-to-hand, but whether one can be built in reasonable time. The answer depends basically on whether or not the problem involves kinds of relationships that have not been established by previous scientific study or, as I shall say, whether or not it involves gaps in fundamental scientific knowledge. Consider, for example, the advertising fable that we used to characterize the operations analytic approach. In that fable the analyst boldly extemporized a model of the relationship of advertising expenditure to sales. It was, of course, a shot in the dark. No one really knows what the relationship in question is. The problem involved a gap in scientific knowledge.

When he encounters such a gap, the operations analyst has a choice of three options: he can proceed on the basis of a bold conjecture, he can undertake the substantive research necessary to fill the gap, or he can abandon the problem as unsolvable at the current state of knowledge. Much of the analyst's skill lies in determining which option to choose in given circumstances. A bold conjecture is refreshing, but an insubstantial foundation for an important decision. Abandoning the project is manifestly distasteful. Undertaking fundamental research entails the usual hazard that it may not be successful, plus an unwelcome delay in arriving at a useful recommendation.

In practice, this third option is frequently chosen and frequently well advised. Much of the work of the practicing analyst is the work of filling gaps in substantive knowledge, just as much of the value of the model-building approach resides in disclosing and defining those gaps. There is much testimony to indicate that the most valuable results of operations research are by-products. Again and again businessmen have

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stated gratefully that they learned more about their business by answering an analyst's questions, supplying data for his model, and checking over the model with him than they did from his final report. (Is the analogy with psychoanalysis a coincidence?) Similarly the substantive research undertaken as part of an operations research project is often of great value, quite apart from the value of the final recommendations.[20] Thus the attempt to construct a model may be worthwhile even in unpromising circumstances.

But not always. Frequently the gaps in knowledge that prevent constructing a reliable model are already perfectly well known to the client, and not of a kind to be filled by short-term research. The advertising fable is, very likely, a case in point. Such gaps in knowledge are frequently what induce the client to call in the operations analyst. If he could fill them, he could solve the problem himself, and his hope is that the magic of operations research will help him to reach a well-founded decision in spite of certain areas of ignorance. Such hopes are doomed to disappointment. Operations research is not a substitute for substantive knowledge, but a way of employing it, nor can the operations analyst be expected to complete scientific knowledge to order as required by his clients.

If gaps in substantive knowledge prevent the formulation of a complete model, clearly the analyst cannot hope to ascertain the optimal decision. He may then address himself to a more modest, but still useful goal, as pointed out by P. M. S. Blackett in one of the earliest and most important papers on operations research methodology [8]. He can seek to discover a policy that is better than the current one, though not necessarily best. This approach is one that economists are schooled in. Blackett recommended that instead of attempting to ascertain the objective function as a function of the various decision variables, the analyst undertake the much easier task of estimating its partial derivatives with respect to each decision variable (essentially the net marginal productivities of the decision variables) in the neighborhood of the currently used values of those variables. If any of those partial derivatives is substantially different from zero (i.e., if any marginal productivity is substantially different from the corresponding marginal cost) then a direction in which current policies can be improved is apparent.

Just as the operations research approach is not peculiarly adapted to solving fresh scientific questions, so it is not well adapted to discovering fresh lines of action or technological innovations (with an exception to be noted below). A range of possible actions or decisions is

[20] For a famous and fascinating illustration see Thornthwaite, "Operations Research in Agriculture" [27, pp. 368-80].

built into the model from the outset; the solution of the model yields the best solution within that range. For example, linear programming yields the optimal combination of the activities listed in the matrix; it will not disclose the existence of some still better activity not foreseen in advance. In short, the technique of operations research is designed to uncover better ways of organizing the use of given resources by means of given techniques; it does not extend the range of technical possibilities.

This is not to say that operations analysts have not been instrumental in technical innovations. They are typically imaginative and resourceful men, unfettered by traditions of which they are frequently unaware, and often do suggest courses of action that would never occur to men schooled in the habits of an enterprise or branch of technology. But the methods of operations research are of little help in the field of substantive invention though the practitioners often do have patents to their credit.

There is, however, one field of operations research that does bear directly on the process of technical invention, namely "requirements studies." In a requirements study an operation is surveyed in order to determine the characteristics of desirable technical innovations. Models are built which incorporate as yet nonexistent hardware and the performance characteristics of the hardware are varied (on paper) in order to ascertain their influence on the over-all operation. Thus a set of specifications for new equipment can be established and the gain flowing from meeting those specifications can be estimated. This type of analysis has been most prevalent in establishing military requirements—the RAND Corporation was studying the usefulness of artificial satellites as early as 1946—and has also been used by commercial airlines in planning for new equipment. Thus studies are now in progress on the usefulness of supersonic passenger aircraft.

The third characteristic of a promising operations research project was that an adequate fund of experience be available to permit statistical estimation of the parameters required by the model. This requirement will be satisfied most adequately by repetitious, even routine, types of operation. Morse and Kimball, for example, state, "Patrol or search is an operation which is peculiarly amenable to operations research. The action is simple, and repeated often enough under conditions sufficiently similar to enable satisfactory data to be accumulated" [32, p. 38]. Nearly all our examples, indeed, have been of this sort. They concerned scheduling a refinery, which is done at least once a month, reordering inventories, similiarly periodic, and so on. In all such repetitive decisions, the necessary statistics are accumulated over time as an administrative by-product if not as part of a formal reporting sys-

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tem. If the requisite statistics are not available, the situation is analogous to that which occurs when there is a gap in scientific knowledge, discussed above, except that gathering statistics is less of a gamble than undertaking to establish a new scientific relationship.

If the problem being studied is nonrepetitive, even the statistical outlook is more doubtful since, after all, statistics require a population from which a sample can be drawn. The statistical characteristic also, therefore, is more likely to be fulfilled at the operating levels of an enterprise than at the highest executive levels, since operating decisions are much less likely than broad policy and strategy decisions to be *sui generis*.

The final characteristic of a promising operations research study was that current operations admit of substantial improvement by means of the kinds of decisions discoverable by studying the operation of a model. This last characteristic, unfortunately, works somewhat in opposition to the first three. If an operation is repetitive, well recorded, intended to serve a well-defined goal, and of a kind in which the influences of decisions on the attainment of the goal do not transcend available technical knowledge, then it is not likely that current practice will fall far short of the most efficient practice attainable. And, indeed, the usual upshot of a linear programming analysis or a transportation problem study is to find a plan that will reduce costs by 2 or 3 or 5 per cent. To be sure, in a large operation, 5 per cent is not contemptible. But neither is it dramatic; and in view of the approximations used in reaching such solutions and the possible errors in the statistical estimates it cannot even be certain that such small savings are genuine rather than paper results. This finding stands in unhappy contrast to the state of affairs during the second world war, on the basis of which Morse and Kimball wrote, ". . . successful application of operations research usually results in improvements by factors of 3 or 10 or more" [32, p. 38]. This is as if a successful operations research project can be expected to treble the capacity of a factory or divide its unit costs by three.

The contrast between the peacetime and wartime yields of operations research is explained by the fact that the second world war was largely a race of technological improvements. Efficient submarines led to the development of airborne search-radar; airborne search-radar induced the invention of snorkel-equipped submarines. Technological innovations followed each other so quickly that a new device was in the field before trial-and-error methods could discover efficient means for employing its predecessor. Operations research proved to be a very effective means for accelerating the discovery of effective ways of using novel equipment. In more stable circumstances, however, the situation is otherwise. Blackett, also relying on wartime experience, wrote ". . . in

the course of repeated operations by many different participants, most of the possible variations of tactics will be effectively explored, so that any large derivatives will eventually be discovered, and given intelligent control, improved tactics will become generally adopted" [8, p. 33]. On the other hand, considerable room for improvement may remain even under fairly stable technological conditions, as the dragline example showed. The explanation in that instance probably lay in the numerousness and the complexity of the decision variables, which precluded efficient exploration of possibilities by unsystematic means.

These considerations suggest that in just those kinds of business operation in which the first three requirements for productive operations research are likely to be met, the requirements for the discovery of efficient methods by more traditional means are also likely to be met, and there may not be very much improvement left for the operations analyst to discover. The major exception to this conclusion is problems of adapting to new circumstances or of employing novel techniques or instruments. In those cases, operations research can often speed up the process of adaptation. An optimal situation for operations research is one in which conditions are changing too rapidly for experience to be assimilated by informal, unsystematic methods, but slowly enough to permit the formulation of a model applicable to both the recent past and relevant future, and to permit the accumulation of the data needed for estimating the parameters of the model.

All in all, conditions auspicious for operations research are more likely to be met at the middling and lower levels of an organization than at the topmost ones: the clarity of objectives, the simplicity of relationships, and the availability of technical knowledge and statistical data all point in this direction. Thus the device of "suboptimization" recommends itself. Suboptimization is defined by Hitch and McKean, its principal expositors, as the ". . . process of choosing among a relatively small number of alternatives by an administrative level other than the highest" [21, p. 172]. More explicitly it is the organizational policy in which the higher echelons of an organization assign tasks to the lower echelons and establish performance criteria for them, and then leave the lower echelons free to perform the tasks in the best way they can as judged by the criteria established. That, after all, is how a market economy works. Suboptimization is a new word for decentralization, and its advantages are the familiar advantages of decentralization. From the point of view of operations research its major advantage is that it enables the relatively manageable problems of detailed operation to be solved separately from each other and from the more intractable problems that arise on the higher executive levels.

The foregoing summarizes the circumstances in which operations

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research is likely to be successful in the sense of disclosing significantly improved policies and practices. But operations research can be successful in other senses also. We have already noted the educative value of collaborating with an analyst and looking at problems from his viewpoint. We have seen that operations research often suggests and sometimes accomplishes valuable substantive research. In many instances the contribution of operations research is to improve the planning process itself, without improving the quality of the plans.

Consider planning petroleum refinery operations, which now is quite prevalently accomplished with the use of programming models. Before programming was introduced, monthly refinery schedules were developed by a planning section in the refinery engineer's department and required about two weeks of computation by highly trained engineering personnel. After a programming system is introduced, the same plans are developed in three or four hours by clerical personnel and computing machine operators under the general supervision of an engineer. The new planning system has at least three advantages over the old one, even though the resultant plans are not appreciably superior to those developed by tedious hand calculations using the same data. First, it is vastly cheaper in terms of both monetary cost and drain on scarce, highly trained manpower. Second, because of its speed, more timely data can be employed. Before the mechanized planning made possible by operations research, the forecasts for, say, the March plan had to be based on data available on February 14; after mechanization the closing date for forecast data becomes February 26. Thus, even where programming does not produce superior plans given the same data, the speed with which it can be performed permits the use of improved data. Third, the probabilities of errors in both arithmetic and judgment are greatly reduced when formalized, mechanized planning supersedes informal, skilled-judgment methods. The programming procedure includes a built-in guarantee that the resulting plan is optimal, avoiding the hankering worry that a misjudgment was made somewhere in a protracted computation.

In more general terms, where plans or decisions are based on large masses of data and complicated interrelationships—where, for example, a large number of operations have to be coordinated—the model developed by an operations research study provides a framework within which the data can be assembled, analyzed, and digested in a swift, mechanical, error-free, and inexpensive way. Such a model makes the planning process itself more efficient and reliable.

Finally, consider the really tough and important problems where there is no objective basis for making a usefully precise evaluation of the consequences of possible actions or policies. Contending with such

imponderables is the primary responsibility of the high executive, a responsibility that cannot be delegated, not even to an operations analyst. Nevertheless, an operations research study of such a problem can help the executive by organizing the data, focussing the issues, bringing out the implications of possible assumptions and hunches, delimiting the range of the inevitable uncertainty. Any detached, analytic, skeptical viewpoint can help clarify such problems, and the analyst has such a viewpoint to contribute.

There is much room for folly, though, when an operations analyst participates in conjecturing answers to unanswerable questions. The executive is under a strong temptation to pass the buck to the analyst, and the analyst is tempted just as strongly to accept it. When there is no "right" decision, there is a tendency to adopt one that can be justified—for who can be blamed for following a recommendation supported by an imposing dossier? And what dossier is more imposing, these days, than an operations research report? Thus the analyst may find that his simplifying assumptions, perhaps made for exploratory purposes, have become the basis for weighty decisions, even decisions important to the safety of the nation.

It is all very well to inveigh against the executive who permits his judgment to be overborne by elaborate calculations that he does not understand. Though the executive must retain the responsibility, he must also accept much on faith, and when his analyst assures him that the recommendations are well-founded, what is he to do? The businessman cannot audit the technical reasoning. Though the analyst can remain detached and impartial as regards the affairs of his client, he is as liable as any man to fall under the spell of his own handiwork. I see no satisfying way to resolve this difficulty. Glen Camp, as we saw, advised analysts to abstain from such dangerous enterprises but also, as we remarked, the analyst can serve usefully in smoothing the way for a decision. The accumulation of experience in the use of operations research will probably help some, particularly by reducing the pressure on the analyst to produce clear-cut recommendations

It appears, in summary, that operations research is best adapted to dealing with routine, semitechnical, quantifiable problems, and that it can also contribute in a larger realm by showing businessmen how to view their problems from a sophisticated scientific standpoint. It has developed powerful methods for solving the day-to-day problems of middle management and, I think, can fairly claim to be an indispensable tool at that level. Operations analysts aspire higher, of course.[21] But when they will attain a special competence in dealing with larger, more conjectural problems is itself a very conjectural question.

[21] See for example Ellis A. Johnson [22] and Russell Ackoff [1].

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We noted at the outset that operations research is dominated by, and takes its style from, men trained in the natural sciences. There is, however, a large handful of practising operations analysts who were trained as economists and, permitting myself a parochial evaluation, these men have contributed to the development of the science far out of proportion to their numbers. The economist comes to operations research with a number of important ideas already instilled, among them an appreciation of the importance of substitution, a sophistication about the objectives of enterprises, an awareness of the importance of marginal trade-offs, and most important, a realization that physical quantities are subsidiary to "values" in a decision process. He also inherits from his training a number of disabilities, including ignorance of the technical side of business and industry and a belief in the existence of production functions. On balance it appears that the older science has more to contribute to the younger than the other way round, as is right and proper. But still we can learn from our juniors, and we fail to do so at our own risk.

The most important lesson operations research has to teach is how much we are asking of businessmen when we ask them to behave "rationally." Even when businessmen would dearly like to do so, it turns out that the most powerful tools of mathematics cannot, for example, help them discover a "rational" inventory policy; and that is only one small part of the businessman's problem. Since the profit-maximizing or risk-minimizing course of action is undiscoverable, he must perforce rely on hunches and rules of thumb. It is by no means clear what rational behavior consists of in such circumstances. On the other hand it turns out that business performance is frequently quite close to the rational optimum for problems of the sort that operations research is able to solve.

Thus the lesson of operations research appears to be a heavy score against the belief that firms maximize anything, either in the short run or the long. Instead we must conceive of actual firms as operating according to certain rules of thumb that would be rational if life were much simpler than it is and that are not disastrous under actual circumstances. It makes one tolerant of such practices as pricing by conventional mark-ups, costing with conventional overhead burdens, and investing in accordance with tried and proven pay-off periods. These practices are what businessmen must follow until operations research provides them with better standards. The success of operations research testifies to the willingness of businessmen to abandon operation by rule-of-thumb when a better alternative becomes available. The best we can say is that businessmen would like to behave "rationally" and are eager to be taught how.

For many purposes of economic analysis the current crude image of the firm, as a responsive extension of the personality of a fully informed, aggressive entrepreneur, is probably adequate. But for some other purposes—the understanding of inventory and investment behavior, for example—we must recognize the firm for what operations research has disclosed it to be: often fumbling, sluggish, timid, uncertain, and perplexed by unsolvable problems. Since its discriminating power is low it responds only to gross stimuli; since its decision processes are uncertain the timing and vigor of its responses are unpredictable. It reacts in familiar ways to the familiar and avoids the novel as long as it dares. We need economic theories that incorporate these ingredients. They will remain valid until operations research has made much more progress against businessmen's problems.

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PATTERNS OF INDUSTRIAL GROWTH

By Hollis B. Chenery*

An increase in per capita income in a country is normally accompanied by a rise in the share of industrial output. The accepted explanation for this relationship is the change in the composition of demand, of which the decline in the share of food (Engel's Law) is the most notable feature. However, this over-all relationship does not necessarily apply to every individual country. Within limits, the changing composition of domestic demand can be offset through foreign trade. A country having a continuing comparative advantage in primary production may therefore reach a high level of income without an increase in the share of industry in total output. Because of the diversity of natural resources, we should not expect to find uniform patterns of growth in all countries.[1]

Evidence from several sources suggests a closer and more pronounced relationship between levels of income and industrial output than would be predicted from the change in demand alone. Kuznets' comparison of some 50 countries [13] shows a marked increase of manufacturing with rising per capita income, as did the earlier analyses of Bean [1] and Clark [2]. The fact that patterns of trade change systematically with rising income levels is equally well established [9]. Historical studies also show considerable uniformity in the rise of industry as growth proceeds [7] [10] [15]. Finally, modern growth theory contains arguments against continued specialization in primary production, stemming from the uncertainty of export demand and the interdependence among sectors of production [14] [16].

In searching for additional explanations of the rise of industry, it is natural to look for systematic changes in supply conditions as well as

* The author is professor of economics, Stanford University. This paper is one of a series of comparative studies of economic growth by the Project for Quantitative Research in Economic Development at Stanford University. I have benefited by helpful comments from Hendrik Houthakker, Kenneth Arrow, Goran Ohlin, Arthur Goldberger, Carmella Moneta, and Don Patinkin. Statistical computations were made by M. Chon, M. Bacharach, and J. Biemans. The research is supported by the Ford Foundation. A preliminary version of this paper was presented at the meeting of the Econometric Society, December 1959.

[1] A strong criticism of the supposed necessity for industrialization to achieve a rising income is given by Viner [18, Ch. 3].

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in demand with rising income. Here two factors are of general importance: (1) the over-all increase in capital stock per worker; (2) the increase in education and skills of all kinds. Since, moreover, the proportions in which labor, capital, and skills can be combined vary from sector to sector, the change in factor supplies causes a systematic shift in comparative advantage as per capita income rises.

The purpose of this paper is to incorporate changes in both demand and supply conditions into a more general explanation of the growth of individual sectors of production, which can then be used to explain the observed patterns of industrial growth. Section I derives "sector growth functions" from a general equilibrium model which allows for changes in the composition of demand and in factor proportions. A simplified version of this model is used in Section II as a basis for regression analyses of production and import data for a large number of countries. In Section III, these results are used to establish the existence of significant growth patterns for all branches of industry. The relative importance of changes in demand and supply is then determined for each sector. The variability of growth patterns among countries is investigated in Section IV, and the importance of size, natural resources, and other factors is indicated. Section V takes up the policy implications of the analysis.

I. *Determinants of Sector Growth*

The assumptions normally used in formal growth models do not explain differences in sector growth rates. The general equilibrium models of Walras, von Neumann, Leontief, Samuelson and others customarily omit the elements which would lead to persistent differences in growth rates: limited natural resources, changing factor supplies, nonhomogeneous consumption functions, economies of scale, and even international trade. These models imply the optimality or even necessity of proportionate expansion of all sectors in the long run, a growth pattern which is only observed when per capita income does not increase.[2]

To use the Walrasian model as a basis for an analysis of growth patterns, several modifications are needed. First, some allowance must be made for the principal factors leading to nonproportional growth rates. The model must next be reduced to a form in which the explanatory variables are measurable characteristics of national economies. Finally, measures of these characteristics that are available on a comparable basis for many countries must be sought.

Since the Walrasian model omits both international trade and intermediate goods, it assumes domestic production to be identical with final

[2] General-equilibrium models are reviewed in Dorfman, Samuelson, and Solow [6].

domestic use. Adding the missing elements gives the following accounting identity.[3]

$$X_i = D_i + W_i + E_i - M_i \tag{1}$$

where:

X_i is domestic production of commodity i,
D_i is domestic final use of i,
W_i is use of i by other producers,
E_i is the export of i,
M_i is the import of i.

Instead of having one determinant of the level of production, we therefore have four: three components of demand and one alternative source of supply. Although these four elements depend ultimately on some of the same explanatory variables, there is a different relation for each. The equation for the level of production will therefore be derived by combining the functions for the four components.

The Walrasian model takes factor supply functions as given and treats production levels, commodity prices, and factor prices as endogenous variables. Equilibrium values of these variables are determined from the simultaneous solution of equations for demand, factor use, and price formation. Total income is the sum of factor returns.

For the present analysis, income per capita (Y) is taken as an explanatory variable, thus avoiding the necessity of predicting income levels from factor inputs. The factor supplies are classified as labor (L), physical capital (K), human skills (S), sector-specific natural resources (R_i), and total natural resources (R). The size of the country, as measured by its population (N), is included as an exogenous variable.

The expectation of some degree of uniformity in patterns of growth is based on the existence of certain similarities in supply and demand conditions in all countries. These may be called "universal factors," which are distinguished from more variable "particular factors." Among the universal factors are: (1) common technological knowledge; (2) similar human wants; (3) access to the same markets for imports and exports; (4) the accumulation of capital as the level of income increase; (5) the increase of skills, broadly defined, as income increases. The present analysis is based on the assumption that these elements are much the same for all countries. From the similarity of the first three universal factors it follows that differences in production costs and commodity prices are determined primarily by differences in factor prices.

[3] To avoid a subsequent change in notation, all variables will be measured on a per capita basis, as is also done in the statistical analysis.

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The general nature of the functions for the four determinants of sector output is as follows:

1. *Final use of each commodity* (D_i) is assumed to be determined mainly by per capita income. Houthakker's study [11] supports this assumption for household consumption, and it is consistent with the evidence for government consumption and investment although here the variation among countries may be larger. Following Houthakker, I assume a logarithmic function, since his results show fairly constant income elasticities for each commodity. The function suggested for final domestic use per capita is then:

$$\log D_i = \log \alpha_{i0} + \alpha_{i1} \log Y \tag{2}$$

where Y is per capita national income, α_{i1} is the income elasticity of demand for commodity i, and α_{i0} is a constant. In this and other equations, the subscript 0 refers to the constant term; the subscript 1, to the income coefficient; and 2, 3 . . ., to other explanatory variables.

2. *Intermediate demand for a commodity* (W_i) depends on output levels in the sectors using it, on the substitutability of other inputs for it, and on the extent of variation in relative prices of inputs. Here also, international comparisons [5] suggest that it is legitimate to ignore price effects in a first approximation and to make the Leontief assumption that producers' demands depend only on their levels of output. The function for intermediate use is then:

$$W_i = \sum_j a_{ij} X_j \tag{3}$$

where the a_{ij} are input-output coefficients.

If there were no foreign trade, equations (1) to (3) would uniquely determine per capita output as a function of the level of income. Specific resource supplies would limit the level of income achievable but would not affect the composition of output.[4]

3. *Imports and exports*. If all countries had the same per capita endowments of natural resources, the five universal factors would produce a regular change in the pattern of imports and exports as income increased. This will be called "Case A." With given resources and a constant ratio of labor force to population, a higher per capita output would come about only through an increase in capital and skills (including organization, etc.). If these both increased proportionately with income, there would be a variation in the ratio of capital and skills to labor, but at a given income level factor proportions would be the same in all countries. Relative prices and the patterns of trade would thus change systematically with rising income.

[4] But the introduction of technological alternatives and of substitution in demand would permit some variation in the pattern of production, even in a closed economy, unless relative prices were the same in countries having the same income levels.

For exports of a given commodity (E_i) I assume a similar demand facing all countries. An export function for Case A can be derived from these assumptions as follows:

(4a) $$K = K(Y); \quad S = S(Y)$$

(5a) $$P_i = P_i(P_k, P_l, P_s) = P_i(K, S) = P_i(Y)$$

(6a) $$E_i = E_i(P_i) = E_i(Y)$$

where all quantities are per capita, P_k, P_l, and P_s are the relative factor prices, and P_i is the commodity price. Under these assumptions, relative factor prices depend only on factor proportions, which in turn depend only on the level of income.

The cost of producing import substitutes would also be determined by equation (5a). At given import prices, domestic prices determine the commodities which can be more economically imported. The volume of imports, however, also depends on the three components of demand. An import function for Case A may therefore be written as:

(7a) $$M_i = \mu_i.(D_i + W_i + E_i)$$

where $$\mu_i = \mu_i(Y, N)$$

Here μ_i is the fraction of total supply that comes from imports. For imported goods, the size of the domestic market has been introduced as an additional determinant of the cost of local production, and hence of the fraction imported, since many such goods are subject to economies of large-scale production.

When the assumption of uniform resources in all countries is abandoned (Case B), these functions become more complicated. Differences in the total supply of natural resources among countries imply a corresponding variation in the capital and skills required to produce a given per capita income. For Case B, equations (4a) must be replaced by:

(4b) $$Y = \phi(K, S, R)$$

Second, differences in sector-specific resources (R_i) as between countries must be allowed for. Relative prices are no longer determined by the level of per capita income alone. The price (or cost) function (5a) may be restated as:

(5b) $$P_i = P_i(P_k, P_s, P_l, P_{r_i}) = P_i(K, S, R_i)$$

Finally, differences in the supply of natural resources affect the terms of trade for different countries (measured by the labor and capital cost of earning or saving foreign exchange) and hence the extent to which it is economical to export or to substitute for imports in the manufacturing sectors.[5] To reflect this fact, R, a measure of the total supply of na-

[5] For example, lack of natural resources causes Japan and Italy to import larger amounts of raw materials and hence to substitute domestic production for imports of

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tural resources, should also be added to equations (6) and (7) under Case B:

(6b) $$E_i = E_i(K, S, R, R_i)$$

(7b) $$M_i = \mu_i(D_i + W_i + E_i)$$

where $$\mu_i = \mu_i(K, S, R, N, R_i).$$

4. *Production levels* are determined from total demand in the same way as import levels, by multiplying by the fraction produced domestically $(1 - \mu_i)$. For both Cases A and B this gives:

(8) $$X_i = (1 - \mu_i)(D_i + W_i + E_i).$$

Using equations (2), (3), and (6a) or (6b), the components of demand can be eliminated from equation (8) to give a function for sector growth containing only the exogenous variables and production levels in other sectors.

For Case A, this function is:

(9a) $$X_i = [1 - \mu_i(Y, N)][W_i(X_1, \cdots, X_n) + D_i(Y) + E_i(Y)].$$

In applying this function, it is convenient to consider output (X_i) as composed of two parts, the "normal" output for the country's size and income level, $\hat{X}_i$, and a deviation from normal, ΔX_i. Then $X_i = \hat{X}_i + \Delta X_i$. Under Case A, $\Delta X_i = 0$ for all i, and $X_i = \hat{X}_i$.

Substituting this expression into equation (3) gives:

(10) $$W_i = \sum_j a_{ij}\hat{X}_j + \sum_j a_{ij}\Delta X_j = \hat{W}_i + \Delta W_i$$

where $\hat{W}_i$ is the normal value of intermediate demand for a given size of country. For Case A, $\Delta W_i = 0$ since $\Delta X_j = 0$ for all sectors. The sector growth function then depends only on income and size:

(11a) $$X_i = [1 - \mu_i(Y, N)][\hat{W}_i(Y, N) + D_i(Y) + E_i(Y)].$$

Under the less restrictive assumptions of Case B, resource differences cause variations in intermediate demand at a given income level, and the growth function becomes:

(11b) $$X_i = [1 - \mu_i(K, S, R, N, R_i)][\hat{W}_i(Y, N) + D_i(Y) + E_i(K, S, R, R_i) + \Delta W_i(\Delta X_i, \cdots, \Delta X_n)]$$

II. *Statistical Estimation of the Sector Growth Functions*

Although either time-series or cross-section data could be used for the estimation of the sector growth functions, the latter have very substantial advantages. For any particular country, it is not possible to separate the effects of universal and particular factors, and technology and trading possibilities change very substantially over a long period.

manufactured goods to an abnormal extent. The opposite may be said of Ceylon and New Zealand.

Among countries, however, size and income level are practically uncorrelated and the effects of the two can easily be separated statistically. Trading and technological possibilities are also much more similar at a given moment in time. Finally, data for international comparisons are more widely available. The estimation of sector growth functions in the present paper is therefore based on data for a number of countries, mainly for years between 1950 and 1956.[6]

Since no satisfactory measure of resources, either specific or in the aggregate, is available for any substantial number of countries, I shall base the statistical analysis on equation (11a), in which the only explanatory variables are income and population. The effects of the other variables in equation (11b) will be evaluated by an analysis of the residual variation.

The regression equations. There are two possible approaches to the use of equation (11a). Given data on the three components of demand, the more accurate procedure would be to estimate a separate function for each element and then to combine the results. However the number of countries for which a breakdown of demand into these three components can be made is relatively small. The alternative of estimating production and imports as single functions of income and size has the advantage of making possible the use of a much larger sample. It is therefore followed here.

A desirable form for the regression equation is suggested by equation (2) for final demand, which is logarithmic. (Intermediate demand, W_i, is a linear combination of all the final demands.) Since preliminary tests also showed that the logarithmic form fitted much better than a linear function for most sectors, I used a linear logarithmic regression equation in which per capita value added depends on per capita income and population:

$$\log V_i = \log \beta_{i0} + \beta_{i1} \log Y + \beta_{i2} \log N \tag{12}$$

where V_i is per capita value added, β_{i1} is the growth elasticity

$$\left(\frac{dV_i}{V_i} \Big/ \frac{dY}{Y}\right)$$

and β_{i2} is the size elasticity

$$\left(\frac{dV_i}{V_i} \Big/ \frac{dN}{N}\right).$$

A similar function is assumed for imports:

$$\log M_i = \log \gamma_{i0} + \gamma_{i1} \log Y + \gamma_{i2} \log N. \tag{13}$$

[6] In a parallel study, [4], the same theoretical framework is applied to the analysis of Japanese growth patterns from 1914 to 1954.

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It will be shown below that the separate estimation of these two equations also yields an estimate of the import ratio in equation (11a), μ_i, as a function of Y and N.

The two elasticities in these equations include both supply and demand effects. Since factor proportions as well as demands vary with rising income, β_1 and γ_1, are called growth elasticities rather than income elasticities. Similarly, the size elasticities, β_2 and γ_2, represent the effects of larger domestic markets both on the cost of production and on the demand deriving from import substitution in other sectors. β_{i0} and γ_{i0} are the values of V and M at $Y = \$100$, $N = 10$ million, which are taken as units of measurement.

The regression analyses.[7] The sample of countries used for the analysis of manufacturing output is given in the first 38 countries of Table 1. These are countries for which both national income data and a postwar industrial census are available.[8] I have used value added instead of total output as the dependent variable because the former is less affected by variations in product mix. The values given for the explanatory variables apply to the year of the industrial census. Results of the regression analyses for fifteen sectors of manufacturing are given in Table 2.

In order to extend the analysis to nonmanufacturing production and to check on the results of the preceding calculations, a second sample containing production data from national income sources was used. This sample includes 13 additional countries for which it is possible to calculate the average breakdown of the national product by major sectors in 1950-1955. Values of the dependent variable in each regression analysis were obtained by applying these percentages to the United Nations estimates of average per capita income in 1952-54. The regression coefficients are shown in Table 3.

For estimating the import function of equation (13), data are available for many more countries, and a sample of 63 was used. Commodities imported were classified according to the categories in the census of production in order to make possible the addition of imports and domestic output in each sector. An average for 1952-54, the years to which the national income estimates apply, was taken for all countries, since it seemed more important to have uniform trade conditions than to match the dates of the industrial censuses. The regressions are given in Table 4, which also includes primary products.

[7] Additional statistical data and discussion of the methodology used are contained in a mimeographed appendix to the present paper, available from the Research Center in Economic Growth, Department of Economics, Stanford University.

[8] Countries whose production data could not be reconciled with the two-digit international classification of industries or for which comparable national income estimates are not available (mainly communist countries) were excluded. Prewar censuses were used for Italy and the United States because the postwar census does not give comparable data.

TABLE 1—BASIC DATA FOR SAMPLES 1 AND 2[a]

Country	Year	Population[b] (N)	Per[b] Capita Income (Y)	Average Share of Major Sectors in GNP, 1950–1955: IA Agriculture	IB Mining	I Total Primary (3+4)	II Industry	III Transport	IV Services	Source
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
1. India	1950	358.29	58	48.4	0.9	49.3	16.6	—	—	(c)
2. Kenya	1956	6.15	66	49.0	0.9	49.9	13.5	7.0	29.6	(c, i)
3. Pakistan	1953	80.06	68	58.8	0.2	59.0	11.2	2.8	27.0	(c, i)
4. South Korea	1956	21.80	74	42.3	1.2	43.5	14.7	1.9	39.9	(c)
5. Egypt	19.0	20.39	112	38.9	2.1	41.0	19.4	5.0	34.6	(c)
6. Ceylon	1951	7.74	117	53.4	0.1	53.5	13.3	7.4	25.8	(c)
7. Rhodesia & Nyasaland	1955–56	7.12	118	18.8	32.3	51.1	15.8	—	—	(g, j)
8. Iraq	1954	4.95	126	23.9	26.4	50.3	19.3	—	—	(f)
9. Peru	1954	9.21	137	32.7	11.9	44.6	19.9	—	—	(c)
10. Honduras	1	1.66	143	52.3	1.6	53.9	14.0	5.0	34.6	(c)
11. El Salvador	[illegible]	1.92	147	52.4	0.5	52.9	—	—	—	(e)
12. Turkey	1950	20.95	156	45.0	1.4	46.4	16.8	7.1	29.7	(d)
13. Guatemala	1946	2.50	164	45.5	0.4	45.9	21.2	—	—	(i, j)
14. Japan	1953	86.70	181	23.4	3.0	26.4	29.7	7.0	43.9	(c, j)
15. Brazil	1950	51.98	184	27.6	0.6	28.2	23.3	10.8	37.7	(e)
16. Mexico	1951	26.54	229	20.2	4.1	24.3	23.3	4.2	48.2	(e)
17. Colombia	1953	12.11	242	39.0	2.6	41.6	16.8	6.4	35.2	(e)
18. Italy	1938	43.60	250	25.1	1.2	26.3	39.6	6.3	27.8	(c)
19. Union of South Africa	1949–50	12.33	280	15.4	12.5	27.9	25.9	8.2	38.0	(c, g)
20. Chile	1952	6.30	285	14.9	5.4	20.3	20.6	7.3	51.8	(c)
21. Costa Rica	1950–51	.85	287	44.3	0.1	44.4	15.0	—	—	(d, j)
22. Lebanon	1955	1.43	327	19.7	0.1	19.8	16.0	4.6	59.6	(c, j)
23. Puerto Rico	1949	2.19	335							
24. Ireland	1953	2.95	423	32.5	1.0	33.5	25.1	—	—	(c, h)
25. Netherlands	1950	10.11	448	12.0	1.0	13.0	41.9	8.3	36.6	(c, h)
26. Argentina	1950	17.19	542	18.1	1.1	19.2	28.7	—	—	(c, j)
27. Germany (West)	1954	49.52	563	9.9	3.0	12.9	47.1	7.7	32.3	(c, h)
28. Israel	1956	1.81	565	12.2	0.6	12.8	28.5	7.7	51.0	(c, j)
29. Finland	1955	4.24	727	24.7	0.2	24.9	41.1	7.2	26.8	(c, j)
30. Norway	1952	3.33	732	14.3	1.6	15.9	36.3	17.0	30.8	(c)
31. United Kingdom	1951	50.30	757	5.2	3.5	8.7	44.9	8.4	38.0	(c)
32. Denmark	1954	4.41	762	20.5	0.2	20.7	36.5	9.0	33.8	(c)
33. Belgium	1955	8.87	824	8.4	4.9	13.3	45.8	9.0	31.9	(c, j)
34. Sweden	1952	7.13	943	8.0	2.0	10.0	48.0	7.0	35.0	(h)
35. New Zealand	1952–53	2.01	958	23.7	0.9	24.6	29.9	8.9	36.6	(c)
36. Australia	1955–56	9.31	972	18.3	2.7	21.0	36.4	—	—	(j)
37. United States	1939	130.88	1,065	5.5	1.7	7.2	34.7	6.1	52.0	(c)
38. Canada	1952	14.43	1,291	12.1	3.9	16.0	38.5	7.6	37.9	(c)
39. Burma	1952–54	19.05	50	43.9	1.4	45.3	14.2	1.7	38.8	(c)
40. Bolivia	1952–54	3.13	55	28.9	25.4	54.3	19.8	—	—	(d)
41. Belgian Congo	1952–54	12.15	70	27.0	21.4	48.4	17.5	8.3	25.8	(c, i)
42. Thailand	1952–54	19.56	80	49.1	1.7	50.8	14.9	4.2	30.1	(c)
43. Ecuador	1952–54	3.46	150	38.9	2.1	41.0	19.4	5.0	34.6	(c)
44. Philippines	1952–54	21.04	150	42.7	1.4	44.1	14.8	2.9	38.2	(c, j)
45. Nicaragua	1952–54	1.17	155	40.5	0.5	41.0	—	—	—	(e)
46. Dominican Republic	1952–54	2.29	160	42.5	0.5	43.0	18.2	—	—	(i, j)
47. Portugal	1952–54	7.99	200	29.1	0.8	29.9	36.9	4.8	28.4	(c, h)
48. Greece	1952–54	7.82	220	35.2	1.1	36.3	24.2	6.5	33.0	(c)
49. Austria	1952–54	6.95	370	15.3	3.0	18.3	50.1	5.1	26.5	(c)
50. Venezuela	1952–54	5.44	540	8.0	27.0	35.0	24.2	—	—	(c)
51. France	1952–54	42.86	740	16.0	1.8	17.8	31.2	9.0	42.0	(c, j)

[a] Sample 1 consists of countries 1 through 38, Sample 2 includes all countries except Puerto Rico. Sectors are defined from the International Standard Industrial Classification as Primary (0, 1) Industry (2, 3, 4, 5), Transport (7) Services (6, 8, 9).

[b] Population and per capita income are given for census years only for countries 1–38 and for 1952–54 for the remainder. Population in millions from U.N. *Statistical Yearbook*. Per capita income in 1953 dollars from United Nations [17] and other U.N. publications.

[c] Production data from U.N. Statistical Office, *Tables of International Comparisons of National Accounts Items, 1950–1955;* (mimeo.) 1959.

[d] Production data from U.N., *Yearbook of National Accounts Statistics, 1958;* 1959.

[e] Production data from U.N., E.C.L.A., *Producto Bruto, Inversion Bruta y Estructura de la Production Industrial, 1958* (mimeo.).

[f] Production data from U.N., *Economic Development in Middle East, 1956–57;* 1958.

[g] Production data from U.N., *Economic Survey of Africa since 1950;* 1959.

[h] Production data from O.E.E.C., *Industrial Statistics, 1900–1957;* 1958.

[i] Production data from Kuznets, [13].

[j] Production data from country sources.

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TABLE 2—REGRESSIONS OF PRODUCTION ON INCOME AND SIZE: MANUFACTURING SECTORS[a]

ISIC No.	Sector	β_0	Growth Coefficients		Size Coefficients		$\bar{R}^2$	$S_{Y.YN}$	No.	Countries Omitted From Sample 1
			β_1	S_{β_1}	β_2	S_{β_2}				
(20-21)	Food and beverages[c]	3.85	1.129	.088	.001[b]	.058	.846	.178	31	(1-3-6-18-25-28-33)
(22)	Tobacco	.51	0.928	.234	.234[b]	.156	.344	.469	32	(1-2-4-14-18-28)
(23)	Textiles	1.00	1.444	.133	.401	.085	.770	.306	38	None
(24)	Clothing[c]	.50	1.687	.127	.065[b]	.083	.837	.267	35	(1-3-6)
(25-26)	Wood, etc.	.35	1.765	.146	.080[b]	.100	.815	.312	34	(1-3-18-20)
(27)	Paper	.04	2.692	.245	.518	.157	.784	.540	34	(6-10-26-28)
(28)	Printing	.32	1.703	.126	.177	.084	.854	.272	32	(1-20-25-26-28-33)
(29)	Leather	.09	1.642	.164	−.026[b]	.103	.743	.372	37	(7)
(30)	Rubber	.06	1.998	.234	.438	.168	.713	.518	32	(1-18-20-23-24-28)
(31)	Chemicals	.51	1.655	.129	.257	.076	.846	.271	37	(15)
(32a)	Petroleum products	.01	2.223	.327	1.040	.222	.650	.742	32	(15-18-22-25-26-28)
(32b)	Petroleum products[d]	.06	1.568	.288	.670	.246	.592	.522	21	(same+1-6-7-10-11 13-20-21-23-24)
(33)	Nonmetallic minerals	.39	1.617	.155	.164[b]	.101	.747	.358	37	(22)
(34-35)	Metals, etc.	.34	2.143	.234	.419	.149	.726	.524	32	(6-17-20-23-24-33)
(36-37)	Machinery, etc.	.09	2.799	.231	.315	.120	.834	.498	30	(6-9-10-17-18-20-24-33)
(38)	Transport equipment	.18	2.327	.263	.256	.165	.717	.580	31	(6-9-17-18-20-23-33)
(20-39)	All sectors	8.83	1.620	.089	.085[b]	.057	.900	.205	38	—
(20-39)	All sectors[e]	11.92	1.441	.069	.199	.045	.931	.145	35	(1-3-6)

[a] Symbols: β_0 is a constant computed for $Y = \$100$ and $N = 10$ million; β_1 and β_2 are the regression coefficients and S_{β_1} and S_{β_2} their standard errors; $\bar{R}^2$ is the coefficient of determination (corrected for degrees of freedom); $S_{Y.YN}$ is the standard error of estimate.

[b] Coefficient not significantly different from zero at 95 per cent confidence level.

[c] Sample excludes India, Ceylon, and Pakistan, for which census covers only establishments having 20 or more employees.

[d] Sector (32b). Eleven zero-entry countries were treated as a separate population and omitted from the sample in order to allow for the existence of a large minimum size of plant. The estimate for the remaining countries is thereby improved.

Growth interpretation of the cross-section results. Historically, the growth of a country takes place in an environment in which trading possibilities and technology are constantly changing. The growth functions derived from cross-section analysis, on the other hand, represent the adaptation of countries at different levels of income to conditions of technology and trade existing at one time. Ideally, they may be thought of as indicating the path that a typical country would follow if its income increased so rapidly that conditions of trade and technology were relatively constant.

Over the past century, the change in the share of major sectors in the national product of the presently advanced countries has been quite similar to the pattern derived from cross-section analysis.[9] Detailed comparisons of production trends in individual sectors have not yet been made for many countries, but a preliminary analysis of growth patterns in the United States and in six Latin American countries[10] showed considerable similarity to the cross-section results given here. Pending further analysis of production series, it seems justifiable to

[9] Kuznets concludes that "the direct evidence on long-term trends in the industrial structure of national product is thus remarkably consistent with that provided by the association of international differences in industrial structure and in level of per capita income" [13, p. 17].

[10] Undertaken by the present author in collaboration with the Economic Commission for Latin America.

TABLE 3—REGRESSION OF PRODUCTION ON INCOME AND SIZE: MAJOR SECTORS[a] (1950–55)

Sector	β_0	β_1	S_{β_1}	β_2	S_{β_2}	$\bar{R}^2$	$S_{Y \cdot YN}$	No.
I. Primary production	46.49	.494	.043	−.090	.032	.751	.122	48
a. Agriculture	38.98	.474	.062	−.082[b]	.045	.574	.173	48
b. Mining	1.79	.935	.227	.129[b]	.166	.244	.635	48
II. Industry	16.95	1.362	.039	.046[b]	.029	.963	.109	48
a. Manufacturing[c] (factory only)	11.92	1.441	.069	.199	.045	.935	.145	35
b. Construction	4.06	1.152	.074	−.055[b]	.051	.882	.180	34
III. Transportation and communications	4.64	1.288	.066	−.048[b]	.053	.918	.161	36
IV. Other services	32.70	1.066	.038	.014[b]	.030	.958	.093	36

[a] Based on average percentage breakdown of national income in 1950–55 from Table I applied to average per capita national income in 1952–54. β_0 is a constant computed for $Y = \$100$ and $N = 10$ million. β_1 and β_2 are the regression coefficients and S_{β_1} and S_{β_2} are their standard errors; $\bar{R}^2$ is the coefficient of determination (corrected for degrees of freedom); $S_{Y \cdot YN}$ is the standard error of estimate.

[b] Coefficients not significantly different from zero at 95 per cent confidence level.

[c] Manufacturing from census data (Table 2).

TABLE 4—REGRESSION OF IMPORTS ON INCOME AND SIZE[a] 1952–1954

ISIC No.	Sector	Income Coefficients			Size Coefficients		$\bar{R}^2$	$S_{M \cdot YN}$	No.
		γ_0	γ_1	S_{γ_1}	γ	S_{γ_2}			
0	Agriculture	1.17	1.396	.138	−.239	.091	.650	.432	60
1	Minerals								
11, 13	Crude petroleum, gas and coal	.07	2.363	.402	−.001[b]	.259	.420	1.081	46
12, 19	Mining	.17	1.563	.177	.075[b]	.117	.668	.450	39
2, 3	Manufactured goods								
20–21–22	Food, beverage, and tobacco	1.36	1.003	.141	−.374	.093	.552	.443	60
23	Textiles	2.05	.555	.119	−.536	.078	.547	.377	62
24	Clothing	.18	.866	.203	−.757	.126	.543	.524	45
25–26	Wood and furniture	.24	1.320	.154	−.406	.095	.677	.393	44
27	Paper	.43	1.118	.068	−.380	.043	.862	.203	56
28	Printing	.03	1.444	.285	−.331	.139	.506	.476	29
20	Leather	.15	1.143	.130	−.470	.084	.689	.361	49
30	Rubber	.24	.578	.118	−.540	.079	.584	.348	53
31	Chemicals	1.18	.956	.079	−.407	.051	.808	.242	57
32	Petroleum products	.88	1.007	.144	−.438	.093	.576	.432	55
33	Nonmetallic minerals	.28	.853	.112	−.478	.075	.649	.337	58
34–35	Metals	.96	1.192	.102	−.228	.064	.754	.300	53
36–37	Machinery	2.28	.964	.115	−.367	.071	.667	.336	55
38	Transport equipment	1.48	.790	.340	−.507	.214	.707	.313	54
	All imports	20.40	.987	.069	−.281	.045	.808	.217	62

[a] The sample includes 14 countries of income less than $100, 15 between $100 and $200, 16 between $200 and $400, 11 between $400 and $800, and 7 over $800.

Import data for 1952–1954 from U.N. *Yearbook of International Trade Statistics*.

Symbols: γ_0 is a constant computed for $Y = \$100$ and $N = 10$ million; γ_1 and γ_2 are the regression coefficients and S_{γ_1} and S_{γ_2} are their standard errors; $\bar{R}^2$ is the coefficient of determination; $S_{M \cdot YN}$ is the standard error of estimate.

[b] Coefficients not significantly different from zero at 95 per cent confidence level.

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interpret the cross-section results as normal growth functions, although experience with cross-section analysis in other fields of economics suggests caution in making use of this hypothesis.

III. *The Process of Industrialization*

Industrialization involves a number of changes in the economic structure, including: (1) a rise in the relative importance of manufacturing industry; (2) a change in the composition of industrial output; and (3) changes in production techniques and sources of supply for individual commodities. The first two will be measured from the regression analyses. I shall then try to determine the relative importance of changes in demand and supply in causing the growth of each industrial sector.

The rising share of industry. The preceding analyses give two separate measures of the rise of industry: (a) the increase in manufacturing alone, determined from census data (Table 2); (b) the increase of "industry" (manufacturing, construction, electric power, handicraft), derived from national income estimates.[11] The growth elasticity of manufacturing (1.44) is higher than that for all industry (1.36), of which it comprises some 75 per cent. The coefficient of determination (R^2) is high for both regressions: .931 for manufacturing alone and .963 for all industry.[12]

The changing shares of the major sectors in the national income are shown in Figure 1, which I interpret as the contemporary pattern of growth. The principal feature of this pattern is the rise in the share of industrial output from 17 per cent (12 per cent for manufacturing alone) at an income level of $100 to 38 per cent (33 per cent for manufacturing alone) at a level of $1000. The share of transportation and communication also doubles over this range, while primary production declines from 45 per cent to 15 per cent. The regression analysis confirms Kuznets' conclusion [13] that the share of services (other than transport) in national product does not vary significantly with the level of per capita income, since the regression coefficient for income is not significantly different from one at a 95 per cent confidence level.

[11] Censuses of manufacturing normally cover establishments of more than four persons and omit handicraft and other very small-scale manufactures. For low-income countries, the latter component is quite important in a few sectors.

[12] If the share of industry is taken as the dependent variable, the regression equation becomes:

$$\frac{V_m}{Y} = \beta_0 Y^{(\beta_1 - 1)} N^{\beta_2}$$

The estimates of the parameters are unchanged in this form, but $\bar{R}^2$ is reduced from .96 to .64 because the variance of the dependent variable is lower. The growth elasticity for the share of industry ($\beta_1 - 1$) is thus .36 with a standard error of .04.

Figure 2 shows the extent to which individual countries deviate from the normal relation between income level and industrial output (countries are numbered as in Table I). The standard error of estimate is equivalent to 28 per cent of output. The extremes are indicated approximately by the two dashed lines corresponding to outputs 50 per cent above and below normal. In a steadily growing economy, industrial output will increase by this amount over a twenty-year period if per capita income grows at 1.5 per cent a year. In these terms, there is

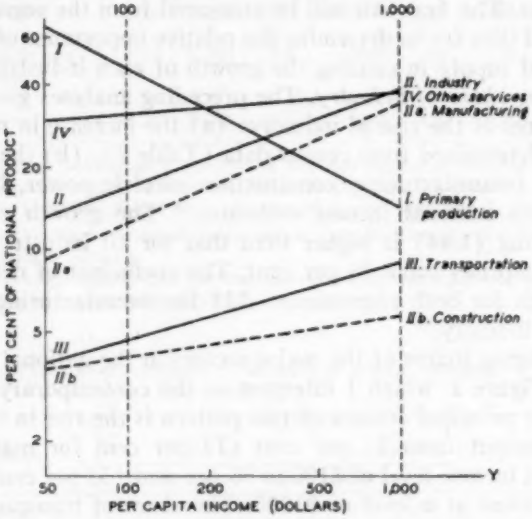

FIGURE 1. THE SHARE OF MAJOR SECTORS IN NATIONAL PRODUCT
(Logarithmic Scale. Population 10 million.)

no country in which industrial development is either advanced or retarded by much more than twenty years.

Figure 2 also shows that the linear logarithmic regression equation fits quite well for the range of incomes from $100 to $1000 to which I shall restrict my analysis. The fit would be somewhat better if size differences were allowed for. As suggested in the appendix, the regression would also fit the highest-income country, the United States, quite well if purchasing power exchange rates were used, since its actual exchange rate is substantially overvalued in terms of European purchasing power. Although no sector of the economy can have a growth elasticity greater than unity over an indefinite income range, the linear logarithmic function gives an adequate description of industrial growth over the range so far experienced in the world.

UNIVERSITY OF MICHIGAN LIBRARIES

The pattern of industrial growth. The change in the composition of industrial output is just as marked as the change in the pattern of output as a whole. In Table 5, the regression equations from Table 2 have been used to determine normal output levels for three groups of industries, classified according to the nature of the demand for their products as: (A) investment and related products, (B) intermediate goods, and (C) consumer goods. As will be shown in the next section,

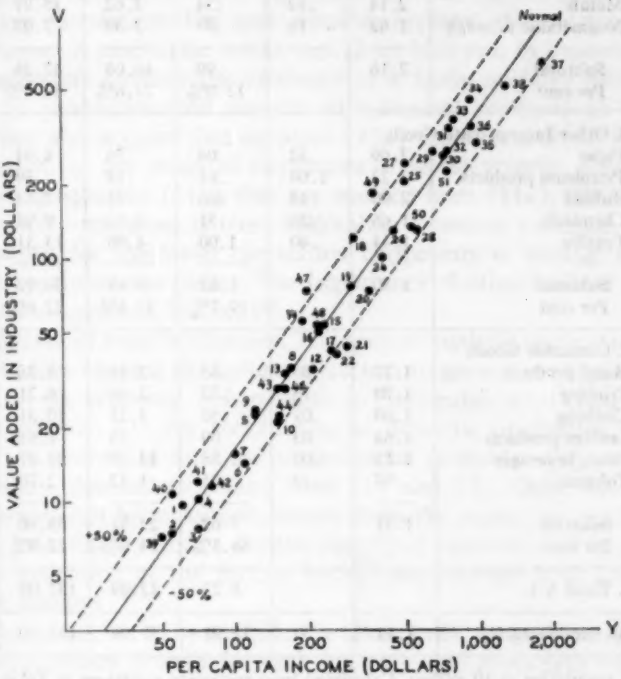

FIGURE 2. INDUSTRIAL OUTPUT AND INCOME LEVEL
(Logarithmic Scale)

there is considerable overlapping in these categories due to aggregation. Apart from metals and nonmetallic minerals, which go predominantly into investment goods, I have kept intermediate goods as a separate category because their growth characteristics are somewhat different from either of the other two groups. The opposite procedure is followed by Hoffman [10], who divides group B fairly arbitrarily between the other two and omits several mixed sectors.

The difference in growth elasticities between investment goods and consumer goods is almost as great as the difference between agriculture

TABLE 5—INCREASE OF MANUFACTURING OUTPUT WITH INCOME*

ISIC No.	Industry Sector	Growth Elasticity β_1	Size Elasticity β_2	Normal Output at: $100	$300	$600	Ratio 600/100
Group A. Investment and Related Products							
36–37	Machinery	2.80	.32	.08	1.84	12.82	151.4
38	Transport equipment	2.33	.26	.18	2.28	11.44	64.6
34–35	Metals	2.14	.42	.34	3.62	15.97	46.6
33	Nonmetallic minerals	1.62	.16	.39	2.30	7.05	18.1
	Subtotal	2.16		.99	10.04	47.28	47.8
	Per cent			12.0%	23.6%	34.5%	
Group B. Other Intermediate Goods							
27	Paper	2.69	.52	.04	.76	4.94	124.1
32a	Petroleum products	2.22	1.04	.01	.13	.59	53.7
30	Rubber	2.00	.44	.06	.53	2.13	35.5
31	Chemicals	1.66	.26	.51	3.16	9.95	19.4
23	Textiles	1.44	.40	1.00	4.90	13.31	13.3
	Subtotal	1.50		1.62	9.48	30.92	14.3
	Per cent			19.7%	22.3%	22.6%	
Group C. Consumer Goods							
25–26	Wood products	1.77	.08	.35	2.46	8.36	23.6
28	Printing	1.70	.18	.32	2.06	6.71	21.1
24	Clothing	1.69	.07	.50	3.21	10.31	20.5
29	Leather products	1.64	−.03	.09	.53	1.65	18.9
20–21	Food, beverages	1.13	.00	3.85	13.29	29.07	7.6
22	Tobacco	.93	.23	.51	1.42	2.70	5.3
	Subtotal	1.31		5.62	22.97	58.80	10.5
	Per cent			68.3%	54.0%	42.9%	
	Total A–C			8.23	42.49	137.00	15.6
20–39	All manufacturing	1.44	.20	11.92	57.99	157.40	13.2

* For a population of 10 million. Calculated from regression equations in Table 2.

and industry. At an income level of $100, 68 per cent of manufacturing consists of consumer goods and only 12 per cent of investment goods.[13] At income level $600, the share of group A has increased to 35 per cent of all manufacturing, while group C has fallen to 43 per cent. Group B maintains a fairly constant share of the total. (The interval from $100 to $600 will be used throughout to illustrate the effects of industrialization.)

The regression of value added on income and population also gives

[13] The errors due to aggregation between groups B and C are approximately offsetting (see Table 6).

UNIVERSITY OF MICHIGAN LIBRARIES

a reasonably good fit for almost all sectors. The coefficient of determination, R^2, is below .70 in only two cases, tobacco and petroleum products, and its median value is .78. A similar result was found for the import regressions, where the median R^2 is .68. Except for the three sectors (food, clothing, printing) in which imports are a very small fraction of total supply, equation (11a) therefore gives almost as good an explanation of imports as of production. The import results are the more striking because a simple regression on either income or size shows a very low correlation. The scale variable explains about a quarter of the variation in production levels, but about half that in imports.

These results confirm the existence of a fairly uniform pattern of change in production and imports of industrial products as income rises. They also suggest that equation (11a) provides a useful first approximation for the study of the causes of sector growth. Among the variables in equation (11b) that are omitted from (11a), the effect of sector-specific resources is most noticeable. Resource variation probably accounts for the lower correlation coefficients in mining, agriculture, and petroleum products. The importance of other omitted factors will be investigated in Section IV.

The causes of industrialization. The present analysis sheds some light on the factors causing the industrial sectors to grow more rapidly than the rest of the economy. It is possible to distinguish three causes of industrial growth: (1) the substitution of domestic production for imports; (2) growth in final use of industrial products; (3) growth in intermediate demand stemming from (1) and (2). Only the first of these factors can be measured directly from the regression results. Additional information from a smaller sample of countries will be used to give an indication of the further breakdown between final and intermediate demand.

The phenomenon to be explained is taken to be the positive deviation from proportional growth for each industry. My procedure will be to calculate the deviation from proportional growth in each sector and then to use the sector growth function to explain the source of this deviation.

For the income range from $100 to $600, proportional growth consists of a six-fold increase in each element of demand, production and imports. From equation (11a), we can write:

$$X^p = \lambda X^0 = \lambda(1 - \mu^0)(W^0 + D^0 + E^0) \tag{14}$$

where the superscript 0 indicates the initial income level, p applies to proportional growth, and λ is the increase in income ($Y^1/Y^0 = 6$). From this definition, the following expression can be derived for the deviation of the actual production level from proportionality:

$$\delta X = (X^1 - X^p) = (1 - \mu^0)(\delta W + \delta D + \delta E) + (\mu^0 - \mu^1)Z^1 \tag{15}$$

Here the symbol δ refers in each case to the deviation from proportional growth; X^1 and Z^1 are the values of production and total supply at the upper income level.

To apply this expression, we first need to measure the normal values of X and Z from the regression analysis. X^0 and X^1 are derived from the value added given in Table 5, using the average ratio of value added to total output (v_i) for each income level:

$$X_i = \frac{V_i}{v_i} \tag{16}$$

Z^0 and Z^1 are taken as the sum of production and imports (with constant population):

$$\begin{aligned} Z^0 &= X^0 + M^0 \\ Z^1 &= X^1 + M^1 = \left(\frac{v^0}{v^1}\right) X^0 \lambda^{\delta 1} + M^0 \lambda^{\gamma 1}. \end{aligned} \tag{17}$$

On the basis of equation (15), the three causes of nonproportional growth can be stated as:

(1) Import substitution: $(\mu^0 - \mu^1)\, Z^1$.

This expression measures the difference between the growth in output with no change in the import ratio and the actual growth.

(2) Nonproportional increases in final demands:

$$(1 - \mu^0)(\delta D + \delta E) = (1 - \mu^0)(D^1 - D^p + E^1 - E^p)$$

where, from (2) $D^1 = D^0 \lambda^{\alpha 1}$

(3) Nonproportional increases in intermediate demand:

$$(1 - \mu^0)\delta W$$

Data for calculating these three components are given in Tables 5 and 6.[14]

The calculation may be illustrated as follows for Sector 23, textiles:

Element	Initial Value	Proportional Growth	Actual Growth	Deviation	Breakdown of δX
Production (X)	2.09	12.54	32.34	19.80	19.80
Imports (M)	2.05	12.30	5.54	−6.76	13.22
Import ratio (μ)	.495	.495	.146	.349	
Total supply (Z)	4.14	24.84	37.88	13.04	
Final Demand ($D+E$)	1.90	11.43	16.31	4.88	2.46
Intermediate Demand (W)	2.24	13.41	21.57	8.16	4.12

[14] The calculation is as follows:

1. Import substitution is measured from the values of X^0 and M^0 in Table 6 by means of equations (16) and (17), using the normal value-added ratios in columns (1) and (2).

The difference between the actual production at income level $600 (32.34) and the six-fold expansion of the initial production level (12.54) is shown as the deviation from proportionality (19.80). The breakdown of this total deviation into its component causes is:

(1) Import substitution	.349 (37.88) =	13.22 (67 per cent)
(2) Growth in final demand:	.505 (4.88) =	2.46 (12 per cent)
(3) Growth in intermediate demand:	.505 (8.16) =	4.12 (21 per cent)

Similar results for all sectors are shown in Table 7. These are summed to give an estimate of the relative importance of the three factors for all sectors of industry.

This analysis shows that the effect of income growth on final demand accounts directly for only 22 per cent of industrialization. To this should be added the intermediate demand deriving from the growth in final demand, which increases the pure demand effects to 32 per cent of the total deviation from proportionality.

The increased share of domestic production in total supply, defined here as import substitution, is more important than the pure demand effects, since it accounts for 50 per cent of industrialization. This total may be subdivided into three components, using a breakdown of the last term in equation (15):

$$\text{(15a)}\quad \underset{(111)}{(\mu^0 - \mu^1)Z^1} = \underset{(1)\ (43)}{(\mu^0 - \mu^1)Z^p} + \underset{(2)\ (37)}{(\mu^0 - \mu^1)(\delta D + \delta E)} + \underset{(3)\ (31)}{(\mu^0 - \mu^1)(\delta W)}$$

The first term represents pure import substitution, that which would have taken place if there were only proportionate growth in demand. The other two elements result from the deviation from proportionality in final and intermediate demands. I have classed them as supply effects, however, because without a change in comparative costs and hence in import proportions they would be zero.

There remains a residual of 18 per cent in group (3), attributable to changes in prices and errors of estimation, which cannot be analyzed without further information. There is probably some net substitution of manufactured goods for other goods and services—e.g., for handi-

2. Initial final demands ($D^* + E^*$) are calculated in column (6) of Table 6 by applying the average proportion of final to total demand in Japan and Italy (which are near the middle of the income range) to the total supply given in column (5). Final demand at the upper income level is determined from equation (12), using the income elasticities of column (13). Since manufactured exports are quite small in this income range, the same growth elasticity is applied to foreign and domestic demand.

3. Intermediate use is measured as the difference between total supply and final demand. Changes in this residual element include the effects of substitution on final as well as intermediate use and of errors of estimation.

UNIVERSITY OF MICHIGAN LIBRARIES

Table 6—Growth of Demand and Supply

ISIC No.	Sector[a]	Value Added Ratios at: $Y=100$ (1)	$Y=600$ (2)	Demand and Supply at Income $100: X^0 (3)	M^0 (4)	Z^0 (5)	(D^0+E^0) (6)	W^0 (7)	μ^0 (8)	Growth Elasticity of: Production β_1 (9)	Imports γ_1 (10)	Total Supply η_1 (11)	Import Subst. $(\beta_1-\eta_1)$ (12)	Final Demand α_1 (13)
A. Investment and Related Products														
36–37	Machinery	.54	.52	.15	2.28	2.43	1.77	.66	.94	2.83	.96	1.55	1.28	1.60
38	Transport equipment	.65	.43	.28	1.48	1.76	1.41	.35	.84	2.55	.79	1.63	.92	(1.60)
34–35	Metals	.49	.44	.69	.96	1.65	.38	1.27	.58	2.21	1.19	1.85	.36	1.60
33	Nonmetallic minerals	.54	.56	.73	.28	1.01	.73	.28	.28	1.59	.85	1.47	.12	1.45
	Subtotal			1.85	5.00	6.85	4.29	2.56	.73	2.24	.97	1.64	1.27	1.59
B. Other Intermediate Goods														
27	Paper	.50	.38	.08	.43	.51	.11	.40	.84	2.84	1.12	1.93	.91	1.60
32	Petroleum products	.64	.35	.02	.88	.90	.20	.70	.98	2.22	1.01	1.08	1.21	.80
30	Rubber	.44	.49	.14	.24	.38	.20	.18	.63	1.95	.58	1.46	.49	1.60
31	Chemicals	.41	.38	1.23	1.18	2.41	.72	1.69	.49	1.66	.96	1.46	.20	.70
23	Textiles	.48	.41	2.09	2.05	4.14	1.90	2.24	.50	1.53	.56	1.23	.30	1.20
	Subtotal			3.56	4.78	8.34	3.13	5.21	.57	1.72	.83	1.38	.34	1.34
C. Consumer Goods														
25–26	Wood products	.48	.41	.74	.24	.98	.62	.36	.25	1.84	1.32	1.75	.09	(1.75)
24	Clothing	.46	.39	1.10	.18	1.28	1.15	.13	.14	1.77	.87	1.71	.06	1.20
28	Printing	.57	.61	.56	.03	.59	.42	.17	.05	1.66	1.44	1.65	.01	1.15
29	Leather	.30	.35	.30	.15	.45	.27	.18	.33	1.57	1.14	1.45	.12	1.20
20–22	Food, beverage, tobacco	.31	.30	13.50	1.36	14.86	12.63	2.23	.09	1.16	1.00	1.15	.01	(1.15)
	Subtotal			16.20	1.96	18.16	15.09	3.07	.11	1.32	1.07	1.29	.03	(1.27)
	Total			21.61	11.74	33.35	22.51	10.84	.35	1.55	.94	1.40	.15	1.36

[a] In each category sectors are ranked in order of the production elasticity (β_1).

Sources:

Column (1) Based on 10 countries having incomes below $150.
Column (2) Based on 7 countries having incomes between $400 and $750.
Column (3) $X^0=V^0/v^0$. V^0 from Table 2; v^0 from col. (1).
Column (4) From Table 4.
Column (5) (3)+(4).
Column (6) Based on average ratios to total supply in Italy and Japan from [5], Table XV.
Column (7) (5)−(6).
Column (8) M^0/Z^0.
Column (9) From Table 2, corrected for change in value added ratio.
Column (10) From Table 4.
Column (13) Based on budget studies in Italy, Ireland, Holland, Finland, Austria, and Japan. For sectors 20–22, 25–26, and 38 and Group C total, the supply elasticity was used, since the final demand estimates were not consistent with these results.

craft products and personal services—which is not reflected in the income elasticities derived from budget studies. There is also some substitution of manufactures—e.g., fuels and fertilizer—for labor inputs in production.

The breakdown by product categories throws further light on the operation of supply and demand factors. In groups A and B, imports provide 64 per cent of the total supply of commodities at an income level of $100. In all sectors except nonmetallic minerals, economies of scale

TABLE 7—THE CAUSES OF INDUSTRIALIZATION[b]

ISIC No.	Sector[a]	Deviations: Import Ratio $\mu^0-\mu^1$	Deviations: Final Demand $\partial D+\partial E$	Deviations: Intermediate Demand ∂W	Deviations: Production ∂X	Effects of: (1) Import Substitution	Per Cent	(2) Final Demand	Per Cent	(3) Intermediate Demand and Substitution	Per Cent
Group A. Investment and Related Products											
36–37	Machinery	.60	20.5	2.6	24.0	22.7	94	1.2	5	.2	1
38	Transport equipment	.65	17.6	4.4	24.8	21.2	86	2.8	11	.7	3
34–35	Metals	.40	4.4	30.5	32.4	17.9	55	1.8	6	12.8	39
33	Nonmetallic minerals	.35	6.2	1.7	8.3	2.5	31	4.5	54	1.2	15
	Subtotal		48.7	39.2	89.5	64.3	72	10.3	11	14.9	17
Group B. Other Intermediate Goods											
27	Paper	.65	11.0	2.2	12.5	10.6	85	1.7	13	.3	2
32	Petroleum	.13	−.4	1.2	.8	.8	98	0	0	(.02)	2
30	Rubber	.50	2.0	.8	3.5	2.6	73	.7	19	.3	8
31	Chemicals	.29	1.8	20.3	19.1	9.6	50	−.9	−5	10.4	55
23	Textiles	.35	4.9	8.2	19.8	13.3	67	2.5	12	4.1	21
	Subtotal		19.3	32.7	55.7	36.9	66	4.0	7	15.1	27
Group C. Consumer Goods											
25–26	Wood products	.13	10.7	6.3	15.8	3.0	19	8.0	51	4.7	30
24	Clothing	.11	3.0	16.6	19.8	2.9	15	2.6	13	14.3	72
28	Printing	.02	.8	7.0	7.6	.2	2	.8	11	6.6	87
29	Leather	.14	.7	2.6	3.0	.9	29	.5	16	1.7	56
20–22	Food, beverage, tobacco	.02	23.9	4.2	28.1	2.6	9	21.7	77	3.8	14
	Subtotal		39.1	36.7	74.3	9.6	13	33.6	45	31.1	42
	Total		107.1	108.6	219.5	110.8	50	47.9	22	61.1	28
									Induced by final demand		10%
									Residual		18%

[a] Sectors are in order of β_1 in each group.
[b] Source: Tables 5 and 6.

relative to the size of the market are substantial, as indicated by the scale elasticity; this is doubtless one of the main reasons for the high proportion of imports. In these two groups, the substitution of domestic production for imports is the cause of the high growth rate, accounting for 70 per cent of the total deviation. For consumer goods, on the other hand, the scale coefficient is not significantly different from zero in any sector and import substitution is a minor factor. The income elasticities derivable from budget studies, however, explain only about half of the

observed growth of consumer industries. In wood products, clothing, and leather goods, a shift from handicraft to factory production is probably equally important. This change in relative costs is analogous to the change in comparative advantage that is the main source of growth in the other two groups.

The growth elasticity of total supply (or total demand) varies relatively much less than the growth elasticity of production. For all industrial sectors, the average growth elasticity of supply is 1.40. Only three (food, petroleum and textiles) are less than this, and only two (paper and metals) are above 1.75. Among the three groups, the range is from 1.64 for investment goods to 1.29 for consumer goods. Intermediate demand in general grows more rapidly than final demand because import substitution requires increased production of intermediate goods; this accounts for the difference between 1.50 for the former and 1.36 for the latter.[15]

These results contradict the usual assumption that changes in the composition of demand are the main cause of industrial growth. If a country has an increase in income with no change in comparative advantage, the analysis suggests that only about a third of the normal amount of industrialization will take place. Changes in supply conditions, resulting from a change in relative factor costs as income rises, cause a substitution of domestic production for imports and, to a lesser extent, of factory goods for handicraft goods and services. These supply changes are more important in explaining the growth of industry than are the changes in demand.

IV. *Variation in Growth Patterns*

The differences in income level alone explain 70 per cent of the variance in the levels of total industrial output among countries and something over 50 per cent for the average sector of industry. I shall now examine the factors responsible for the remaining variation.

The most satisfactory method of determining the effect of other variables is to include them in the regression analysis. This requires an adequate measure, direct or indirect, of the theoretically relevant variable. Only for market size, for which population provides a satisfactory indicator, has this been possible, although several indirect measures of resource endowments were tested. A serious obstacle to the analysis of the remaining variation in industrial output is the existence of several sources of bias in the data.[16] The effects of other elements—factor pro-

[15] An allowance has been made for substitution phenomena in group C.

[16] The main sources of bias are discussed in the appendix.

UNIVERSITY OF MICHIGAN LIBRARIES

portions, income distribution, and national policies—will therefore be indicated in less precise ways.

Effects of market size. Recent theoretical discussions [12, 14, 16] have given considerable emphasis to market size as a determinant of industrial growth. An important by-product of the present study is a quantitative measure of the net scale effect for each sector of industry.

Market size is increased by a rise in either income level or population.[17] For final products, total demand is given by equation (2): $DN = \alpha_0 Y^{\alpha_1} N$. The demand increases proportionately with population but generally more than proportionately with income, since α_1 is greater than 1 for most manufactured goods. If income level is held constant, however, population may be taken as an indicator of the net effect of market size.

When there are economies of scale in production, an increase in market size lowers costs and thus permits the substitution of domestic products for imports. An increase in size also affects output indirectly by increasing the intermediate demand from other industries which experience a substitution of domestic production for imports. This dual effect of size is indicated in the sector growth function, equation (11). The net relation between per capita output and population that has been estimated above therefore reflects economies of scale throughout the economy, both in the sector itself and in its customers. Only when intermediate demand is negligible can the result be inputed entirely to import substitution in the sector in question.

The quantitative effect of size on industrial output is shown in Table 8, which has been computed from the regression equations with the income level held constant at $300. The table includes only the industries for which the scale coefficient is significant at a 95 per cent confidence level, but the coefficients are also positive in all but one of the remaining six sectors. Industries having significant scale effects produce about 40 per cent of manufacturing output at an income level of $300 and 57 per cent at $600.

Although the elasticity of output with respect to size is only .20 for manufacturing as a whole, the relevant range of variation is substantial. An increase in population from 2 to 50 million causes manufacturing output per capita to nearly double and the sectors having significant economies of scale to more than triple. Beyond some point, market size

[17] The use of national population as a measure of the market area must be qualified by the geographical location and trade policy of the country. In Western Europe, both geography and trade liberalization favor an expansion of industrial markets beyond national borders. This is much less true in Latin America, Asia, or Africa, where protection of new industry is the predominant policy and transport facilities are much less developed. The quantitative significance of these factors will be considered below.

should have less effect,[18] but at the income level of $300 chosen here, economies of scale are probably significant up to a population of 100 million or more in most of these industries (a market equal to about 10 million people at U. S. income levels). In Latin America, where markets for manufactured goods do correspond quite closely to national boundaries, Brazil may be expected to have twice as much industrial output per capita as the countries of Central America because of scale factors alone. The actual differences are even greater, since the large countries of Latin America have predominantly positive residuals from

TABLE 8—NORMAL INCREASE OF MANUFACTURING OUTPUT WITH SIZE OF COUNTRY[a]

ISIC No.	Sector	Size Elasticity (β_2)	Normal Output at Population of:			Ratio Column (4) to (2)
			2 mil.	10 mil.	50 mil.	
		(1)	(2)	(3)	(4)	(5)
23	Textiles	.401	1.95	4.90	9.36	4.80
27	Paper	.518	.33	.76	1.76	5.33
28	Printing	.177	1.37	2.06	2.73	1.99
30	Rubber	.438	.20	.53	1.08	5.40
31	Chemicals	.257	1.79	3.16	4.88	2.73
32/b	Petroleum products	.670	.07	.34	1.01	13.73
34–35	Metals	.419	1.37	3.62	7.05	5.15
36–37	Machinery	.315	1.11	1.84	3.06	2.76
38	Transport equipment	.256	1.27	2.28	3.47	2.73
	Subtotal		9.46	19.49	34.40	3.64
20–39	All Manufacturing[b]	.199	42.05	57.99	79.92	1.90

[a] For all sectors where β_2 is significant. Income level held constant at $300.
[b] Predicted from the aggregate regression equation which omits countries 1, 3, 6.

the regression equations while the Central American countries have predominantly negative residuals (see Table 9 on p. 648).

Effects of income distribution. On theoretical grounds, we should expect variations from the normal income distribution to affect the levels of demand and production for commodities having income elasticities substantially different from unity. The effects of abnormally unequal income distributions are notable in countries such as South Africa, Kenya, and Peru, in which predominantly European communities have much higher per capita incomes than the larger native communities. An indication of the importance of this phenomenon is given by applying equation (12) to each community separately and adding the results.

[18] There are too few very large countries in the sample to demonstrate this fact statistically.

UNIVERSITY OF MICHIGAN LIBRARIES

Assuming two communities, the following formula shows the ratio of true normal output, $\bar{V}$, to that calculated from the average income level:

$$\frac{\bar{V}}{V_e} = w_A\left(\frac{Y_A}{Y}\right)^{\beta_1} + w_B\left(\frac{Y_B}{Y}\right)^{\beta_1} \tag{18}$$

where

V_e is the output calculated from the average income level,
w_A is the fraction of population in community A,
w_B is the fraction of population in community B,
Y_A is the per capita income in community A,
Y_B is the per capita income in community B,
Y is the average per capita income for the whole country.

For commodities having unit or zero income elasticity, income distribution will therefore have no effect.

To illustrate this result, assume an average income of $125, income levels of $300 and $50 for A and B, and 30 per cent of the population in A, which may not be far from the situation in Peru. From equation (18), the ratio of true to calculated values is 1.84 for a sector having a growth elasticity of 2.0 and 1.30 for an elasticity of 1.5.[19]

Although I have not been able to estimate the income-distribution effect in quantitative terms, it is probable that it explains the positive deviations in many sectors of manufacturing shown in Table 9 for Rhodesia-Nyasaland, South Africa, Kenya, Brazil, and Peru.

Factor proportions. Although natural resources are now given less importance as determinants of the rate of growth than they once were, their effect on the pattern of growth is undeniable. A common expectation [12, p. 351] is that countries lacking resources will turn to manufacturing at an earlier stage in their development in order to make up for their lack of primary products for export and domestic use. When they are successful, the result is to substitute capital and skills for natural resource inputs.

A quantitative analysis of this process is beset by many difficulties. Attempts to improve the regression analyses by introducing additional variables representing resource endowments have so far met with only limited success because of the inadequacy of the available measures. Since no single measure of resource endowments has proved satisfactory, a more promising approach is to compare countries which on several criteria can be classed as having relatively low or relatively high resource inputs for their income levels. This procedure has been

[19] In this case, a better prediction of output comes from taking the high-income community alone if the growth elasticity is above 1.4.

Table 9—Deviations from Predicted Output and Imports[a]
(Logarithms)

Country and Region[b]	Output of Major Sectors		Output of Manufacturing Total	(+)	(−)	Per Cent Positive	Total Imports	Manufactured Imports	(+)	(−)	Per Cent Positive
	Primary	Industry									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		(9)	(10)
Africa											
2. Kenya			.152	11	3	79	.251				
2. Kenya and Uganda	−.093	−.008					.002	13		0	100
41. Belgian Congo	−.053	.068					.326	11		0	100
7. Rhodesia & Nyasaland	.025	−.023	.567	11	3	79	.408	1		0	100
5. Egypt	.016	.015	−.154	4	11	27	.052	12		2	86
19. Union of South Africa	.030	.006	.241	12	3	80	.246	9		2	82
Average of region	.015	.012	.202			66	.214				94
Asia											
39. Burma	−.138	.019					.029	6		2	75
1. India	.055	−.001	−.467	0	9	0	−.143	6		2	75
3. Pakistan	.106	−.165	−.431	3	7	30	−.223	2		8	20
4. S. Korea	−.077	−.021	.211	13	1	93	−.001				
42. Thailand	.015	−.035					.038				
6. Ceylon	.074	−.117	−.303	3	7	30	.200	6		4	60
8. Iraq	.118	−.010	−.064	8	7	53		9		6	60
44. Philippines	.096	−.138					−.047	10		2	83
14. Japan	−.020	.100	.333	12	2	86	.048	4		10	29
12. Turkey	.194	−.136	−.062	8	7	53	−.201	8		6	57
22. Lebanon	−.239	−.135	−.168	4	9	31	.074	4		8	33
Malaya							.402	14		0	100
28. Israel	−.290	.019	−.013	8	2	50	.048	3		10	23
Average of region	−.009	−.052	−.107			47	.019				56
Latin America											
40. Bolivia	−.109	.185									
Haiti							.069	2		1	67
46. Dominican Republic	.012	−.014					−.200	1		1	50
13. Guatemala	.051	.047	−.100	4	11	27	−.065	4		8	33
10. Honduras	.079	−.110	−.063	5	8	38	−.255	1		10	91
45. Nicaragua							−.126	4		10	29
11. El Salvador			.075	5	10	33	−.097	3		8	27
9. Peru	.018	.043	.097	11	2	85	−.160	5		8	38
15. Brazil	.033	−.027	.193	12	1	92	.055	7		2	78
16. Mexico	−.068	−.006	.010	11	4	73	−.031	8		6	57
Paraguay							−.104	7		7	50
17. Colombia	.160	−.152	.119	10	2	83	−.354				
43. Ecuador	−.007	.016					−.059	8		6	57
Jamaica							−.256	0		6	0
20. Chile	−.096	−.108	−.013	6	3	67	.030	0		1	0
Panama							−.205	2		8	20
Cuba							−.029	6		7	46
21. Costa Rica	.118	−.171	−.076	5	10	33	.109	7		4	64
Uruguay							−.202	4		10	29
26. Argentina	−.025	−.023	.108	11	1	92	−.095	2		9	22
23. Puerto Rico			−.062	4	8	33	−.186				
50. Venezuela	.223	−.098					.087	6		3	67
Average of region	.030	−.032	.026			60	−.099				43
Europe											
48. Greece	.056	.035					−.090	9		5	64
47. Portugal	−.048	.234					−.031	7		7	50
18. Italy	.062	.160	.176	7	1	88	.052	8		6	57
49. Austria	−.132	.272					.006	7		7	50
24. Ireland	.120	−.027	.004	7	5	58	.145	12		2	86
27. Germany	−.137	.156	.228	13	2	87	.059	9		5	64
51. France	.079	−.079					−.049	5		9	36
25. Netherlands	−.198	.176	.027	9	3	75	.352	12		0	100
30. Norway	−.068	.038	.046	10	5	67	.123	9		5	64
29. Finland	.112	.104	.031	12	3	80	−.052	3		6	33
32. Denmark	.059	.038	−.097	7	8	47	.080	10		4	71
Iceland							−.066	5		9	36
31. United Kingdom	−.213	.068	.132	8	7	53	.138	10		4	71
33. Belgium	−.090	.106	−.295	1	9	10	.202	14		0	100
34. Sweden	−.186	.105	−.023	7	8	47	.032	10		4	71
Switzerland								6		5	55
Average of region	−.042	.099	.022			61	.056				63
Other											
35. New Zealand	.167	−.084	−.170	5	10	33	.006	6		6	50
36. Australia	.157	−.019	−.038	4	11	27	−.015	10		4	71
38. Canada	.116	−.056	−.042	4	11	27	.098	8		6	57
37. United States	−.059	−.204	−.147	0	15	0	−.421	2		12	14
Average of region	.095	−.091	−.099			22	−.083				48

[a] Sources: Cols. (1) and (2): Deviations computed from Table 3.
Col. (3): Deviations in all manufacturing from Table 2.
Cols. (4) to (6): Distribution of positive and negative deviations from Table 2.
Col. (7): Deviations in all imports from Table 4.
Cols. (8) to (10): Distribution of deviations in imports of manufactured goods from Table 4.

[b] Countries are given in ascending order of per capita income within each region. The number preceding each country indicates its place in Table 1.

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followed in another study of this body of data [3]. One result was to identify a characteristic pattern of import substitution. The high-resource countries (such as New Zealand, Denmark, and Costa Rica) tend to have relatively low domestic production of machinery, transport equipment, chemicals, textiles, and metals, and to compensate by high imports of these commodities, financed by high primary exports. The low-resource countries (such as Japan, Italy, Germany, and the United Kingdom) do the opposite; they offset low exports and high imports of primary products by high domestic production of these same groups of manufactured goods. Machinery is the sector most sensitive to resource endowment; a large proportion of it can be supplied more economically by imports when a country has a comparative advantage in primary production and exports. For this sector at least, the inclusion of a measure of trade in primary products in the regression equation does give a significant improvement in the results.

Regional differences. The residuals from the regression equations can be used to test the effects of a variety of other factors, such as climate, government policy, or cultural elements, on the levels of production and imports. A simple procedure is to classify countries according to the given characteristic and then to determine whether the variance among the means of the groups is significantly greater than would be expected from the variance within the groups. The present analysis takes up the effect of a regional grouping which reflects variation in several of these factors.

Deviations from the predicted values in all three sets of regressions are summarized in Table 9 for five regions. These are geographical groupings except for the classification of the United States and Canada with Australia and New Zealand. The variance ratio (F-test) was computed for the deviations in primary production, industrial production, and total imports by region. It shows that for primary output the regional difference is not significant, for industrial production it is significant at a 95 per cent confidence level, and for imports it is probably significant also.[20] The following reasons for these regional effects may be suggested.

Industrial output in Europe has an average positive deviation of .10 in logarithms, equivalent to 25 per cent above the predicted values. Asia and the four "other" countries (New Zealand, Australia, Canada, and the United States) have average negative deviations equivalent to 13 per cent and 23 per cent respectively. For the two high-income groups the difference is probably attributable mainly to the difference

[20] The chi-square test for deviations in imports is significant at a 99 per cent level (grouping Asia with Africa and "Other" with Europe), but an F-test of the deviation in regional means is only significant at 80 per cent.

in per capita resource endowments and to the earlier start of industrial growth in Europe. The negative deviations in industry in Asia are concentrated in a few sectors, since almost half of the sector observations have positive deviations. Colonial policies and cultural factors may be suggested as possible explanations of the small lag in industrial development in Asia, but it may merely be that the normal relationship is not a linear one. The normality of industrial output in the African countries contradicts the latter hypothesis, however.

In imports, Europe and Africa have positive deviations averaging 14 and 64 per cent respectively, while Latin American countries have an average negative deviation of 25 per cent. Protectionism in Latin America and relatively free trade in Europe and Africa provide a partial explanation, but the relatively high level of primary exports from Africa (and correspondingly hig' nports) also reflect its rich resource endowment relative to a low income level. (Latin America would have been much closer to this pattern thirty years ago.) Among Asian countries, the effects of colonial development of primary exports are only apparent in the high import levels of Ceylon and Malaya, and the regional average is close to normal. There is no normative implication of this result, however, since the regressions only indicate the prevailing conditions.

Although the regional breakdown does indicate the effect of some set of factors other than size and income on levels of industrial production and imports, the deviations are not sufficiently pronounced to reject the basic assumptions of similar wants and production possibilities on which the study is based. The regional associations do suggest that an attempt to identify other systematic influences on production and trade may be fruitful.

V. *Implications for Resource Allocation*

The association between industrialization and rising income tells us very little about the factors causing the rise in income itself. What the analysis does indicate is the pattern of resource allocation that normally accompanies a rise in income. Growth is likely to be accelerated by anticipating desirable changes in resource use and retarded by institutional arrangements or government policies that inhibit such changes.

Some of the specific conclusions which affect resource allocation policies are the following:

1. When allowance is made for variations in size of country, there is a well-defined growth pattern for individual sectors of the economy. Deviations from this normal pattern are smallest for services, agriculture and most manufactured consumer goods.

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2. The greatest variation in output levels is in industries producing machinery, transport equipment, and intermediate goods, where economies of scale are most important. Differences in factor endowments are reflected mainly in the variation in proportions of imports and domestic production in these sectors.

3. Where a country deviates considerably from the normal output pattern, there is some evidence that lagging sectors of industry are likely to grow more rapidly than normal and tend to approach the normal pattern. A tendency of lagging sectors to approach normal levels was found in a preliminary study of six Latin American countries and also in Japan [4] and Israel.

4. Although additional evidence is needed to prove the importance of economies of scale, the association of output with market size in a majority of sectors strongly suggests their significance. For all but the largest underdeveloped countries, the introduction of regional markets would substantially increase the expected level of industrial production.

5. The explanation of industrialization in the twentieth century that emerges from this study is rather different from the nineteenth-century pattern. In Rostow's terminology, leading sectors are likely to be industries in which import substitution becomes profitable as markets expand and capital and skills are acquired. Even in Japan, the most successful of the low-income countries in increasing industrial exports, import substitution accounted for nearly 40 per cent of the rise of industry (from 23 per cent of GNP to 33 per cent between 1914 and 1954) as compared to less than 10 per cent for exports.[21]

6. Development policies are usually guided as much by analogy to other countries as by an explicit analysis of the factors peculiar to a given situation. While the present analysis has focused on the similarities in the pattern of growth, it has also revealed the substantial variation that exists and the need to separate particular from universal factors. An analysis of the part played by comparative advantage and other particular factors in a given country must therefore be added to a knowledge of general growth patterns to arrive at the best allocation of resources.

Appendix: Sources of Bias in Estimation

Biases in the statistical estimates arise from systematic errors of measurement, from conceptual differences between the statistical measures available

[21] This result was derived in [4], which uses an analytical framework similar to the present study. In this case, it was possible to calculate the effects of substitution and technological change as a separate element, since the input-output coefficients at the end of the period were known. The low proportion attributable to changes in demand (16 per cent) is due to the fact that per capita income somewhat less than doubled in this period.

and those desired, and from the estimating procedures used. The following sources of bias may be significant:

1. The only verifiable error of measurement is the variation in coverage of the industrial censuses. In India, Pakistan and Ceylon the difference is most pronounced, since establishments employing less than 20 workers are omitted, whereas most of the other censuses include those above four or even less. These three countries have been omitted in two sectors where small plants are important. In the low-income countries, there is also some evidence of a lower coverage of the establishments that are nominally included. The apparent non-linearity of the production-income relations below $100 per capita may result from this fact.

2. National income serves here as an indicator of both market size and relative factor costs. In neither case is a conversion at the official exchange rate appropriate; it is used only for lack of a feasible alternative. A conversion using purchasing power for consumer goods would give a better indication of market size. Gilbert and Kravis [8] give such rates for four European countries and the United States, and they have also been estimated for a few other countries.

The following formula gives the corrected ratio of observed to calculated values in manufacturing ($\overline{V}_m{}^o/\overline{V}_m{}^c$), allowing for the exchange rate applied to all income (P_y) and for the exchange rate applied to manufactured goods (P_m):

$$\log\left(\frac{\overline{V}_m{}^o}{\overline{V}_m{}^c}\right) = [\log V_m^o - \log V_m^c] + [\log P_m - \beta_1 \log P_y] \tag{19}$$

Taking the average of the four European countries in [8] as unity, the value of P_y for the U. S. is about .7 and for Japan [19] is about 1.33. The average wholesale prices of all manufactures are almost equal in Japan and the U. S. at the official exchange rate [19]. The price of manufactured goods must be close to unity in both countries, since their export trade is mainly in manufactured goods. On this assumption, the correction for the exchange rate would be of the following order of magnitude:

	$\log V_m^o - V_m^c$	P_m	P_y	$\log P_m$	$-1.44 \log P_y$	$\log\left(\frac{\overline{V}_m^o}{\overline{V}_m^c}\right)$
U. S.	−.147	1.0	.7	0	.223	+.086
Japan	+.333	1.0	1.33	0	−.178	+.155

Although comparable corrections have not been made for other countries, it is probable that they would be considerably less than those indicated for Japan and the United States in most cases. This calculation suggests that the lower share of manufacturing in total output in the United States may be the result of differences in relative prices of manufactured goods and services, resulting

from differences in the growth of productivity, rather than from factors on the demand side.

3. Differences in value-added ratios, resulting from protection, monopoly or technological differences, are a third probable source of bias. Their effect is also included in equation (19). Although there does not seem to be a systematic relation between value-added ratios and income level in most sectors, there is some evidence that prices of manufactures are higher in the less developed countries. To the extent that there is a negative correlation between prices and income level, the growth elasticities will be understated.

4. Although the coefficients were estimated for each sector independently of the others, the total of the predicted values of output from Table 3 is close to total national income. The total is about 1 per cent high at an income of $100 and 2 per cent high at an income of $1000 and population of 10 million, but for other population sizes the difference may be larger.

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CREDIT CONTROLS AND FINANCIAL INTERMEDIARIES

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Two important views with regard to credit controls have been expressed in recent discussions of monetary policy. One view holds that existing controls of commercial banks are inadequate and recommends that new control techniques be investigated [11, pp. 588-606]; the other points to structural changes in the economy which have undermined the effectiveness of traditional credit controls and recommends that bank-type controls be considered for nonbank financial intermediaries [7, pp. 536-38] [13, p. 879] [3, pp. 81-82] [2, p. 223]. Both agree that monetary policy is insufficiently effective; both lead to policy recommendations for additional controls. It is the purpose of this paper to examine (with special reference to credit restriction) some of the proposals for additional bank controls and for extending bank-type controls over nonbank financial intermediaries.

I. *Federal Reserve Control and Liquidity*

At present, the only control over banks is the authority of the Federal Reserve to set reserve requirements for member banks. This authority is not in fact nearly so critical for control of commercial banks as is generally assumed. Legal reserve requirements are not the only and not even the most important means by which the central bank can influence commercial bank policy. Banks would hold liquidity reserves even without legal compulsion; and the Federal Reserve can have a powerful impact on the banking system without recourse to its authority over reserve requirements and even without direct dealings with banks. In the absence of legal reserve requirements, the Federal Reserve could tighten credit by open-market operations even if it were to refrain from making sales of securities to commercial banks. Open-market operations to mop up excess liquidity of the public could affect the banks because the latter are the repository for an important part of the public's liquidity. However, bank deposits are not the only source of funds for security purchases by the public. People could pay for the securities by liquidating near-liquid assets which are held in the form of the indirect debt of some other financial institutions. Although the

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public may tap nonbank sources of funds to buy the securities, the banks could not avoid the impact of the open-market operations. This is because assets from other sources must be converted into money before being used to pay for securities.

The main problem for credit policy is that all the instruments of credit policy together do not ensure sufficient control of bank liquidity.[1] The major way in which the banks can offset the pressure on their reserves is by tapping idle funds and thereby increasing velocity. To enhance the effectiveness of credit policy, additional controls have been considered: on bank assets, on bank lending, and on bank holdings of securities.

The proposal to control the volume of bank credit by basing reserves on bank assets rather than on bank deposits is not substantially different in effect from the present system of basing reserves on deposits. Moreover, it would not touch the problem of bank-induced increases in velocity; for banks would still be in a position to offset pressure on their reserves by tapping idle funds. The effectiveness of asset reserve plans which are designed to induce banks to keep their most liquid assets by providing for differential reserves against different types of assets would depend on the comparative profitability of different portfolio combinations. During a period of economic expansion, such plans might delay but would not be likely to prevent velocity-increasing portfolio shifts in banks.

The proposals to control bank lending may also be inadequate. Although most bank-induced increases in velocity are brought about by an expansion of bank loans based on security liquidations, bank lending per se is not at the heart of the control problem. If bank lending were controlled by law, bankers might increase their purchases of new securities. They might also develop loan substitutes which use new credit forms. For example, they might develop a kind of private-placement mechanism as a substitute for large business loans. Under such circumstances, central bank control of bank lending might not be enough to control bank-induced increases in velocity. Moreover, the activation of idle funds could take place, despite rigid control of loan volume and in the absence of suitable loan substitutes, if banks were to develop facilities for increasing the marketability of loans. Loan marketability (or shiftability) exists when the present holder of a loan can transfer it to another holder. At present, a bank can increase velocity when it acti-

[1] A study of clearing house banks in New York City revealed very wide fluctuations in liquidity ratios when liquid assets were defined as excess reserve balances at Federal Reserve Banks, one-year or shorter-term government securities, call loans to brokers and dealers, bankers acceptances, and commercial paper. Between 1947 and 1952, these ratios ranged from less than 20 per cent to more than 45 per cent of adjusted demand deposits [8, pp. 42-44].

vates idle funds by selling government securities to nonbank investors and uses the funds to make loans to customers. If there existed a market for loans similar to the market for securities, bankers could sell their loans rather than their securities. A degree of marketability for loans already exists for government-guaranteed mortgages because there is a secondary market for those mortgages.

The proposal for a variable secondary reserve composed of government securities of any maturities also might not assure adequate control for a variety of reasons. First, banks could shift from one to another maturity of U. S. government security. This shift among different government securities could be just as inflationary as a shift from securities to loans. Second, the authority to vary secondary reserves would no doubt be granted within fixed limits and, in a particular situation, these limits might not be adequate. Third, the plan could not prevent the unstabilizing consequences from liquidation of the uncontrolled part of the security portfolio. Fourth, a variable secondary reserve would not affect the bank's ability to tap idle funds by selling existing loans to nonbank holders in order to make new loans or to buy new securities and thereby to increase velocity. Finally, the authority of the Federal Reserve to impose a variable secondary reserve would probably cover member banks only. This could undermine the effectiveness of the plan not alone because of its limited coverage but also because the Federal Reserve authorities might be inhibited from exercising their powers fully lest member banks leave the Reserve system.

The common weakness which is shared by the foregoing proposals, and which would limit their effectiveness, is the fact that they do not fundamentally impair the excessive liquidity of the banking system. As an alternative to more controls, it may be more efficient to strike at the sources of bank liquidity by altering the environment in which banks operate, i.e., by a structural change instead of by additional controls on the existing structure. In principle, both loans and securities are potential sources of liquidity for banks. At present, however, the superior shiftability of securities makes them more important than loans as a source of bank liquidity. The largest component in the security portfolios of commercial banks consists of the securities of the federal government. Bank holdings of government securities for short-term liquidity reserves are not troublesome for credit policy. The potentially troublesome government securities are those purchased with funds which are temporarily surplus on the customer loan market and which are held for income purposes pending a revival of loan demand. As a rough approximation, let us assume that the former category consists of Treasury bills and certificates while the latter category consists of longer-term securities. This division could be the basis of a possible

structural change in lieu of additional controls. For example, instead of a variable secondary reserve requirement, the same purpose might more effectively be served by requiring that, with the exception of short-term liquidity reserves, all other bank-eligible government securities be nonmarketable.[2] This would preserve for banks most of the advantages of government security holdings while preventing their potentially unstabilizing effects. The volume of reserves could still be altered by open-market operations and the potential money supply could thereby be kept under control. In addition, bank-induced increases in velocity would be significantly reduced.

An important source of bank liquidity at the present time, government security liquidation, would be precluded except at the discretion of the monetary authority operating through the discount window. This structural change would be more effective than imposition of a variable secondary reserve requirement because it would be the equivalent of a 100 per cent secondary reserve of government securities held (other than those constituting short-term liquidity reserves), effective for nonmember as well as member banks of the Federal Reserve, without the disturbing effects on the banks of arbitrary changes in the percentage of required secondary reserves, without the possibility of banks tapping idle funds and activating them in the interests of some new government issue, and fully effective without compulsion on banks to buy and hold government securities.

It should be stressed that this structural change could not stabilize the velocity of money. Banks could still tap idle funds by liquidating corporate or municipal securities and, to a very small extent, by liquidating loans. In addition, the public could tap idle deposits by new primary security issues.[3] However, this change could substantially reduce the degree of uncertainty which exists for the monetary authority about the impact of credit policy upon the liquidity position of banks. The main disadvantage for individual banks would be the greater inflexibility in portfolio management.

II. *Reserve Requirements and Nonmonetary Intermediaries*

This section examines the proposals to extend bank-type controls over nonbank financial intermediaries. A reserve requirement could be

[2] The absence of legal compulsion to hold these securities would help to raise their yields above the levels that might prevail in a completely captive market and perhaps above rates on marketables depending on the availability of suitable alternative outlets for surplus funds. The present volume of government securities in bank portfolios suggests that suitable alternative outlets for funds could not readily be discovered. For maximum effectiveness, the supply of Treasury bills would also have to be limited.

[3] This amount of slack may even be desirable. Lawrence Ritter has argued that: "Within limits, monetary policy can make a positive contribution toward . . . [controlled expansion], in part because of—rather than in spite of—fluctuations in velocity" [9, p. 129].

most readily imposed on the intermediaries whose liabilities are nearest to money, the financial intermediaries in the savings deposit industry. The most important ones are commercial banks—with respect to time deposits—(TD), savings and loan associations (SLA), and mutual savings banks (MSB). Although the savings deposit industry operates de facto on a fractional reserve basis, only commercial bank time deposits are subject to legal reserve requirements imposed by the Federal Reserve. In analyzing the effects of extending the authority of the central bank to set reserve requirements, it will be assumed that any legal reserves which might be required for the deposit intermediaries would be held as deposits in commercial banks.

Direct effect of legal reserves on savings deposits. The deposit intermediaries,[4] like the commercial banks, can raise the velocity of money (defined as demand deposits and currency). In order to be effective, therefore, credit policy which is aimed at the reserves of the deposit intermediaries must ultimately influence their velocity-affecting activities. However, it is possible for credit policy to affect velocity without affecting the volume of intermediary deposits. Thus, if intermediary reserve requirements were raised, velocity would be affected but the volume of intermediary deposits would be unchanged.[5] It is also possible for an intermediary to change the amount of credit it grants (i.e., to increase the turnover of its credit) without making any change in the volume of its outstanding credit. For example, the volume of outstanding credit of a deposit intermediary would not be affected though the velocity of money would be increased, if the intermediary were to tap idle demand deposits by selling a loan to a nonbank holder and using the proceeds to lend to a new borrower. In a similar way, by a sufficient increase in the turnover of its loans, it would also be possible for a deposit intermediary to raise velocity while the volume of its outstanding credit actually fell. In view of these possibilities, in the discussion which follows, the actual variation in the volume of credit corresponding to different assumptions about changes in reserves may only approximate the variations in credit-granting activities.

[4] In this paper, hereafter, "deposit intermediaries" or "intermediaries" refers to SLA and MSB but does not include TD.

[5] Donald Shelby found only a small effect on liquid assets of the public (defined as the public's holdings of demand and time deposits and the shares of savings and loan associations and mutual savings banks) from assumed wide variations of reserve requirements on intermediary deposits. He concluded that ". . . if there has been any serious erosion of the powers of the Federal Reserve, it cannot be attributed to its inability to control directly the reserve ratios of intermediaries" [10, p. 539]. It should be noted, however, that variations in intermediary credit outstanding, and therefore also in velocity, as a result of variations in assumed reserve requirements for intermediaries are consistent with no changes whatever in liquid assets (as defined above). Moreover, as shown in the text, velocity can also vary without any change occurring in the outstanding volume of credit of the intermediaries.

A given increase in reserve requirements (assuming no excess reserves) would force asset liquidation of earning assets in both banks and deposit intermediaries. Higher reserve requirements on time deposits would not lead to an automatic reduction in time deposits but (*ceteris paribus*) it would lead to a reduction in demand deposits and to a corresponding reduction in bank credit. Thus, asset liquidation by banks restores reserve levels by reducing the volume of both assets and liabilities by a multiple of any increment which is impounded in legal reserves. Higher reserve requirements on other savings deposits (MSB and SLA) would not directly reduce the volume of those deposits as long as the public's propensity to hold those deposits remained unchanged.[6] In deposit intermediaries, asset liquidation restores reserve levels by altering the composition of assets and by leaving the liability structure unchanged. Hence, the volume of outstanding credit in those institutions would decline, but only by an amount equal to the dollar increase in reserve requirements.

Although a given increase in legal reserve requirements would, in the absence of excess reserves, have a strong impact on the credit volume of commercial banks, it would have a comparatively weak impact on the credit volume of deposit intermediaries. This important difference in the effects of a given change in reserve requirements is not based on different assumptions about the level of reserves which might be required for bank deposits (including demand deposits) and for intermediary deposits. The difference exists because, following an increase in reserve requirements, intermediaries could increase the volume of intermediary reserves whereas the banking system could not increase the volume of bank reserves.[7]

In spite of these differences, the monetary authority could manipulate legal reserve requirements for intermediaries so as to bring about a contraction in the volume of intermediary credit comparable to the multiple contraction of credit in commercial banks. However, in order to achieve this similar effect, reserve requirements in the intermediaries would have to be increased substantially more than any increase in the legal reserve requirements for commercial banks. Under those circumstances, it may be misleading to treat legal reserve requirements for intermediaries as if they were simply an extension of a bank-type control. It is likely that the difference in degree of change required would be widely regarded as tantamount to a different kind of control.

[6] This differs from the situation with demand deposits. Even if the public's propensity to hold demand deposits were unchanged after higher reserve requirements on demand deposits, the banks would be unable to sustain that volume of demand deposits, assuming no excess reserves.

[7] Except by borrowing from the central bank: and that borrowing is subject to control.

Indirect and secondary effects. Could credit policy reach the deposit intermediaries indirectly by the application of pressure on the required reserves for commercial bank demand deposits? The answer would depend on whether (and if so, how much) the pressure on commercial banks could alter the volume of intermediary deposits. This would depend on the public's propensity to hold different kinds of deposits. Higher reserve requirements for demand deposits would initially alter the composition of the liquid asset holdings of the public by forcing a reduction in the volume of demand deposits. If the public wished to maintain the same distribution in its liquid asset holdings as before the higher reserve requirements on demand deposits, the pressure on demand deposits could be transmitted to the intermediaries and could force a contraction of their credit as well. On the other hand, if the public were disposed to maintain the level of its savings deposits, despite a decline in demand deposits, the pressure on demand deposits would not be transmitted to the deposit intermediaries.

The possibility that raising reserve requirements on intermediaries might have a secondary effect on the intermediaries could be analyzed in a similar way. Higher reserve requirements for intermediaries would not directly affect the volume of total deposits, demand and intermediary. However, the higher reserve requirements would reduce the volume of demand deposits available to the public because the intermediaries would impound additional demand deposits to hold as reserves. This change in the composition of the liquid asset holdings of the public could lead to a secondary effect on the intermediaries under certain assumptions about the public's propensity to hold different kinds of deposits. These propensities and their consequences for credit policy are examined in greater detail in the following section.

III. *Reactions to an Excess Demand for Money*

A restrictive credit policy on the part of the Federal Reserve would bring about an excess demand for money. A well-known textbook has explained the effects of an excess demand for money in this way: "When the demand for money balances is in excess of the supply of money, the community attempts to satisfy its excess demand for money by decreasing its expenditures for output, by borrowing more to add to its money balances, or by decreasing the supply of loan funds" [4, p. 272]. For our purposes, however, this description is insufficiently detailed because it does not look inside the aggregate of the "community." Everyone is not equally affected by, nor reacts in the same way to, the excess demand for money. This is more than a matter of degree: some may alter the composition of their assets in an effort to overcome the

shortage of money (availability effect); others may experience no shortage of money but may alter the composition of their assets because interest rates have changed (rate effect).

The reactions to an excess demand for money may also reflect the particular manner in which the tight money policy is carried out, because different methods of achieving the same initial quantitative credit impact could have different availability and rate effects. For credit policy, it would be important to know the specific reactions to an excess demand for money, because these reactions serve to communicate the impact of a restrictive credit policy to different financial institutions and to influence which particular institutions will be affected. Certain reactions to an excess demand for money could even have the result of converting a restrictive pressure into an expansionary effect for particular intermediaries. It is the purpose of this section to examine the most important reactions to an excess demand for money, to identify some of the major considerations which influence the public's choices among the alternatives, and to show the implications of different reactions for credit policy. The various ways in which the public can react to a tight credit policy are discussed in terms of the variables which are of special interest for credit control, viz., demand deposits, time deposits, SLA and MSB deposits, and securities.[8]

Interest Rate Effect

Shifts away from money. One possible rate effect of an excess demand for money could be the result of unequal movements of different interest rates. For example, concern has been expressed that higher interest rates associated with a tight money period, far from inhibiting credit extensions of the (deposit) intermediaries, may even stimulate their growth by raising the opportunity cost of holding cash balances [3, p. 50]. This is a possible reaction, but it needs to be restated to take account of differences among individuals. Unequal interest rate movements are not felt equally by all individuals, and, for some individuals, the rise in intermediary deposit rates may not signal a corresponding rise in the opportunity cost of holding cash balances. The increase in interest rates that could be earned on intermediary deposits may not induce bank borrowers to shift away from demand deposits because banks commonly require them to maintain larger compensatory balances (or banks enforce normal balance requirements more strictly) when credit becomes tight. Similarly, higher intermediary deposit rates may not induce some depositors to shift away from money. Periods of rising interest rates accentuated by a tight money policy are often associated with inflation. When inflationary rises in bank costs

[8] The possible reduction of expenditure for output is ignored in this analysis.

lead to higher service charges on demand deposit accounts, the net advantage of shifting from demand deposits to other assets is reduced for some depositors.

A significant shift away from money to intermediary deposits may be held down for another reason. The fluctuation of intermediary deposit rates is a less sensitive (and, therefore, for some depositors, a less compelling) measure of the changing opportunity cost of holding demand deposits than the fluctuation of rates on other liquid assets, such as U. S. Treasury bills. Table I compares the fluctuations of Treasury bill rates, time deposit rates, and intermediary deposit rates from 1951-1957. It is clear that, during a period of generally rising interest rates, movements of the intermediary deposit rates were generally quite sluggish compared with movements of the Treasury bill rate.

TABLE 1—INTEREST RATES ON TREASURY BILLS AND VARIOUS SAVINGS DEPOSITS, 1951–1957

Year	Treasury Bills (per cent)	Time Deposits (per cent)	SLA Share Accounts (per cent)	MSB Deposits (per cent)
1951	1.55	1.1	2.7	2.1
1952	1.77	1.1	2.8	2.4
1953	1.93	1.1	2.9	2.5
1954	.95	1.3	2.9	2.6
1955	1.75	1.4	3.0	2.7
1956	2.66	1.6	3.1	2.8
1957	3.27	1.8	3.3	3.0

Sources: Treasury bill rates are reported in *Federal Reserve Bulletins;* other rates are given in [14, p. 22]. The savings deposit rates are effective interest rates, i.e., ratios of interest paid to the volume of savings deposits.

A shift from demand deposits to time deposits is another possible rate effect of an excess demand for money after a restrictive credit policy raises the opportunity cost of holding demand deposits as measured by the rate of return on time deposits. The decision about this shift would be influenced by the same considerations which have been described in connection with the shift from demand to intermediary deposits. In addition, a shift to time deposits could be inhibited by an effective ceiling rate on time deposits. However, for several years prior to 1957, the ceiling seems to have exerted no undue pressure on the time deposit rate in member banks.

A restrictive credit policy would be weakened for different reasons if it were to induce a shift away from demand deposits either to intermediary deposits or to time deposits. A shift from demand deposits to time deposits would per se transmit no pressure to the deposit intermedi-

aries. Moreover, the shift would offset Federal Reserve pressure on commercial banks because time deposits carry lower reserve requirements than demand deposits. Similarly, a shift from demand deposits to intermediary deposits would per se put no pressure on the commercial banks and it would offset a restrictive credit policy by the increase in intermediary credit. Although bank credit could expand when demand deposits are shifted to time deposits, because reserve requirements differ for time and demand deposits, the expansion of intermediary credit when demand deposits are shifted to intermediary deposits would not depend on lower reserve requirements for intermediary deposits than for demand deposits. The intermediaries could expand their credit, following a shift from demand to intermediary deposits, regardless of the level of fractional reserves which they might be required to hold. This net increase in credit could occur because the shift of deposits would not force the banks to reduce the volume of bank credit while it would enable the intermediaries to increase the volume of intermediary credit.

Shifts among nonmonetary assets. It has also been suggested that an excess demand for money might cause a shift from time deposits to intermediary deposits (as well as from demand deposits to intermediary deposits) because: "During periods of tight money, the rise in interest rates on primary securities enables nonbank financial intermediaries to raise the rates that they pay on their own obligations relative to the controlled rates paid by commercial banks on deposits" [6, p. 105]. The strength of this reaction depends upon the degree of responsiveness of deposit placement, as between different types of deposit institutions, to changing rate differentials among different deposit intermediaries, as well as the extent to which a tight money policy actually widens the interest-rate differences.

Deposit placement as between institutions is a function of many variables [1, pp. 1-22] and the interest rate is only one of these.[9] Moreover, a tight money policy may not always widen interest-rate differences among the deposit institutions. For example, except during the recession of 1953-1954, the general level of interest rates moved upwards between 1950 and 1957. During those years of rising primary security rates, interest rates paid by the uncontrolled intermediary changed relative to those paid by the controlled intermediary—but not in the expected direction. Table 2 shows that the ratio of SLA interest-dividend rates to time deposit interest rates actually fell from 278 per cent in 1950 to 157 per cent in 1957. During the same period, the ratio

[9] For small depositors, comparatively small rate changes are hardly worth bothering about because the disutility of shifting the account may exceed the utility of the negligible absolute interest difference.

of savings bank rates to time deposit rates fell from 222 per cent in 1950 to 143 per cent in 1957.[10]

There are many reasons why the higher primary security rates associated with a tight money policy may not widen interest rate differences very much among savings deposit institutions. On the one hand, rates in the controlled intermediary may rise when the ceiling rate is not seriously limiting. This appears to have been the case until 1957 when the ceiling rate was raised. In addition, rates in the uncontrolled intermediary may not rise very much as compared with the increase in the general level of primary security rates.[11] This is because interest-dividends are paid from earnings and not even gross earnings rates are tied directly to primary security rates. Most of the assets in the intermediaries' portfolios would carry the old rate levels; only new credit would reflect new rate levels. Moreover, the new credit may not reflect the full increase in the general level of primary security rates because the deposit intermediaries deal with only a few kinds of primary securities. Their most important asset is usually mortgages; and mortgage rates, even on conventional mortgages, are less flexible than other primary security rates, such as those on government securities. Furthermore, the higher interest rates charged to borrowers may not be promptly reflected in higher dividend rates because, among other reasons, dividend rates are changed at discrete intervals. Finally, the dividend rates

TABLE 2—DEPOSIT INTERMEDIARY RATES AND TIME DEPOSIT RATES, 1950–1957

Year	SLA Rate/Time Deposit Rate (per cent)	MSB Rate/Time Deposit Rate (per cent)
1950	278	222
1951	236	191
1952	245	218
1953	255	227
1954	223	200
1955	207	193
1956	188	175
1957	157	143

Source: Calculated from [14, p. 22]. The ratios are based on effective interest rates.

[10] During this period when time deposit rates were rising relative to intermediary deposit rates, the ratio of SLA deposits to time deposits was rising and the corresponding ratio for savings banks also rose somewhat. Specifically, the former rose from 40 per cent in 1950 to 78 per cent in 1957; the latter rose from 57 per cent in 1950 to 59 per cent in 1957 (reaching a high of 64 per cent in 1956). These ratios are calculated from [14, p. 13].

[11] For example, during the years 1950 through 1958, SLA (effective) dividend rates ranged between 2.5 per cent and 3.5 per cent; MSB interest rates moved between 2.0 per cent and 3.2 per cent. During this same period, Treasury bill rates fluctuated between .04 per cent and 3.23 per cent [14, p. 22] (*Fed. Res. Bulls.*).

which might be paid by deposit intermediaries may not be predictable solely from a knowledge of the changes in primary security rates because dividends depend on costs, on reserve ratios, and on deposit growth in the intermediaries as well as on their earnings from higher interest rates on primary securities.

Table 1 showed that rate movements both on time deposits and on intermediary deposits are generally smaller than the rate movements on another liquid asset, Treasury bills. These uneven rate movements suggest another possible rate effect of an excess demand for money, the shift from savings deposits to securities. This is an important alternative for savers and the slowdown in the accumulation of time deposits at commercial banks during the fourth quarter of 1958 has been attributed by one financial newsletter partly to the shift into Treasury bills of sizable deposits by corporate and institutional investors [15, p. 1]. The same source concluded that: ". . . the temptation to relate commercial bank losses in time deposits to savings and loan gains and to attribute such shifts to the rate differential between these institutions is probably an oversimplification of the events which are taking place" [15, p. 1].

During several years of generally rising interest rates on primary securities, interest rates paid by an uncontrolled intermediary (SLA) have not risen relatively to the rates paid by a controlled intermediary (commercial bank time deposits). This fact does not preclude the possibility that, with effective ceiling rates on time deposits, interest rate differences may widen in the expected direction during some future tight-money period and bring about a shift from time deposits to intermediary deposits. With respect to the implications for credit control, if an excess demand for money were to induce a shift among nonmonetary assets from time deposits to intermediary deposits, the level of demand deposits would increase because intermediary reserves are held mostly in commercial banks. The net increase in demand deposits would be smaller than the amount shifted from time deposits to intermediary deposits (assuming the banks to be fully loaned) because reserve requirements are different for demand and time deposits. Hence, the volume of bank credit would decline. However, under present effective reserves held by banks and by nonbank intermediaries, the decline in bank credit could be more than compensated by the increase in intermediary credit.

When an excess demand for money raises the interest rates on liquid primary securities more than those on savings deposits, the public may decide to convert some of its savings deposits into securities. A shift either from time deposits or from intermediary deposits to securities would be the equivalent of a shift from time deposits or intermediary

deposits to demand deposits when the system is fully loaned. The credit control implications of this shift are discussed below in connection with the availability effect of an excess demand for money.

Availability Effect

The possibility that a tight money policy might induce a direct shift from savings (both time and intermediary) deposits to demand deposits is generally ignored in the literature. This neglect is probably because changes in the substitutability of savings and demand deposits are usually related to interest rate changes.[12] The shift is entirely plausible, however, when viewed as an availability effect of a tight money policy. Those directly affected by the shortage of money, either because they are denied credit from the usual sources or because the credit is very costly owing to higher borrowing rates, can restore money balances in this way. The credit control implications of a shift away from savings deposits to demand deposits will be different depending on which kind of savings deposit is transferred. When commercial bank time deposits are shifted to demand deposits, the volume of demand deposits would increase but the higher reserve requirements for demand deposits would force a contraction of outstanding bank credit. Hence, the net increase in demand deposits would be less than the amount which is shifted from time deposits. Moreover, this shift would have no direct effect on the volume of intermediary credit outstanding. By contrast, when the availability effect of a restrictive credit policy takes the form of a shift from intermediary deposits to demand deposits, the volume of commercial bank credit would not be affected but the volume of intermediary credit would decline.

To conclude this catalogue of possible reactions, the availability effect of an excess demand for money may take the form of a shift from securities to demand deposits. This well-known reaction to a tight money policy would not alter the quantity of money (though it would probably raise velocity) nor would it directly affect the intermediary credit volume. However, the shift would probably raise interest rates on primary securities and bring about some of the effects described earlier in this section under Interest Rate Effect.

IV. *Credit Control and Deposit Intermediaries*

In order to be effective, a policy of credit restriction must impair the liquidity of the intermediaries. The liquidity of commercial banks can be impaired in two ways: (a) by reducing their ability to tap idle funds

[12] The more usual expectation is for shifts from time deposits to demand deposits as a result of a fall in interest rates, i.e., during easy money periods. Cf. the implicit statement to this effect in [12, p. 545].

by means of asset liquidation, and (b) by reducing the amount of their liquidity reserves available for credit extension. The possibilities of impairing the liquidity of nonbank savings deposit institutions can be analyzed along the same lines.

Impaired asset shiftability. Commercial bank liquidity could be impaired by permitting banks to hold only nonmarketable government securities, except for secondary reserves. Would a similar regulation for the savings deposit intermediaries have a comparable effect on the liquidity of the intermediaries? The answer depends on whether the sale of government securities is potentially an important source of funds for the intermediaries. Between 1951 and June 1959, the SLA government security account increased from $1.6 billion to $4.4 billion [5, p. 1169]; hence, the sale of government securities was not a net source of funds for SLA during this period. During the same period, although government securities were a source of funds for loans or other investments by MSB, the liquidation of those securities was not a very important source of funds. While the MSB government securities declined from $9.8 billion to $7.3 billion, the corporate and other security accounts increased from $2.6 billion to $5.6 billion and the mortgage account increased spectacularly from $9.7 billion to $23.8 billion [5, p. 1168].

For many years, the intermediaries have been able to dispose of their investable funds between mortgages and corporate securities, and they have not sought government securities as a repository for temporarily surplus funds [12, pp. 550-51]. Moreover, mortgages are normally the most important asset for the deposit intermediaries—and mortgages are the sole loan asset for which there is something of a secondary market. Accordingly, in the deposit intermediaries, loan shiftability is an important alternative to security shiftability as a source of funds. Under these circumstances, it is unlikely that credit policy towards the intermediaries would be made very effective by requiring the intermediaries to hold only nonnegotiable government securities.

Reduced liquidity reserves. An impairment of intermediary liquidity by reducing the volume of intermediaries' reserves available for credit extension could proceed along three lines. The first is direct control in the form of legal reserve requirements for the deposit intermediaries. For reasons explained earlier, the direct effect of a given change in reserve requirements on the credit volume of an intermediary is much smaller than the corresponding effect on commercial banks. The intermediaries would experience a strong secondary impact from higher reserve requirements for the deposit intermediaries if the public could be induced to shift from intermediary deposit accounts either into demand deposits or into securities. However, these shifts would not be

more likely to occur because reserve requirements were imposed on the intermediaries than they would be if an excess demand for money were brought about without controls on the intermediaries.

A second possible way to reduce intermediary reserves or at least to retard their growth would be to equalize the competition between commercial banks and deposit intermediaries. One widely suggested proposal would subject all savings deposit intermediaries to uniform ceiling rates on interest-dividends paid to savers. The purpose of this control would be to limit the shift from demand and time deposits to intermediary deposits and to encourage the shift from intermediary deposits to securities when there is an excess demand for money. By itself, this policy is unlikely to be very effective. The competitive importance of rate differences as well as the extent of the rate differences among savings deposit institutions can be overemphasized. In addition, the rate difference between time deposits and other intermediary deposits is not fully explained by the ceiling rate on time deposits. In part, this is because the ceiling rate has not always been effective. Aside from this fact, however, there are differences in the economic circumstances between commercial banks and the deposit intermediaries which would make rate differences both possible and also, in the absence of rate regulation, likely [1, pp. 1-22]. Accordingly, a ceiling rate set at the same level as the ceiling on time deposit rates would simply permit more intensive product competition by those institutions which now pay more than the ceiling rate. Product competition could then substitute for rate competition and could undermine the attempt to extend the impact of an excess demand for money to the intermediaries. The alternative, which is to set the uniform rate ceiling at the highest level of rates paid by any of the savings deposit institutions, would be ineffective because it would leave the existing rate competition unchanged.

A third possible way to reduce the liquidity reserves of the intermediaries would be to impair their liquidity indirectly instead of introducing new controls. In this approach, pressure could be applied either directly or indirectly on the public's liquidity. In either case, the monetary authorities would concentrate on the reactions of the public to an excess demand for money. If the public were to react to pressure on its liquidity either by transferring intermediary deposits to demand deposits or by converting intermediary deposits to securities (necessarily passing through the stage of demand deposits), the intermediaries would lose liquidity reserves. In this indirect way, the intermediaries could be obliged to conform to the requirements of a restrictive credit policy.

The indirect approach to the control of deposit intermediaries could

take a variety of forms. For example, as compared with direct regulation of the intermediaries, a restrictive credit policy might put greater pressure on intermediary liquidity if the commercial banks were permitted to hold only nonmarketable government securities (except for short-term liquidity reserves). As long as government securities continue to provide a source of funds for bank lending, the commercial banks will often be able to relieve the pressure upon the public's liquidity when there is an excess demand for money. If government securities held by banks were nonmarketable, the banking system could not use the Treasury's securities to frustrate central bank credit policy and the public would have to look elsewhere for relief. These circumstances increase the probability that the availability effect of an excess demand for money would take the form of a shift from near-money to money. Any shift of near-money away from savings accounts at the deposit intermediaries would reduce the liquidity of the intermediaries and force them to contract their credit volume.

Another example of an indirect way to reduce intermediary liquidity is suggested by the Treasury's experience in 1959 with the $2.2 billion issue of 5 per cent notes with a maturity of four years and ten months, the so-called "Magic Fives" [16, p. 4]. In that operation, the Treasury was able to attract a large volume of deposits from savings deposit institutions with an appropriate rate magnet on marketable government securities.[13] That experience suggests an important way for the Treasury to support monetary policy; it also suggests an important supplement to the Federal Reserve's own credit policy. Federal Reserve open-market sales to the public put pressure on commercial banks by absorbing the public's holdings of demand deposits. The open-market sales could also reduce intermediary liquidity if the public were to pay for the securities by liquidating their intermediary deposit accounts. To a very great extent, the intermediary deposit accounts are held by small savers who are not accustomed to buying marketable government securities. The probability that open-market sales would tap intermediary deposits could be increased in various ways, such as greater efforts at public education designed to encourage small investors to think of marketable government securities as possible substitutes for intermediary deposit accounts. It would also be necessary to reorganize the government securities market which is not at present geared to handle small transactions. It may be useful, too, to experiment with new techniques to supplement open-market sales of securities, such as public subscription sales with securities supplied from Federal Reserve portfolios. Institutional modifications along these lines would raise

[13] It would undoubtedly require more experimentation to discover how often and under what circumstances this kind of operation could be repeated successfully.

highly technical problems about the internal organization of the government securities market and would deserve careful study by experts in that area.

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THE RADCLIFFE REPORT AND EVIDENCE

A Review Article

By JOHN G. GURLEY*

A drama critic recently wrote of an evanescent Broadway play: "It was paved with good intentions, and, like most pavements, it was trodden underfoot." While for myself, I hesitate to call this an epitaph to the Radcliffe *Report,* it describes pretty well the initial responses of the *Report's* horde of critics, some of whom judged it the most monumental flop of the 1959 season, and most of whom felt that it had nice stage settings and all that, but was terribly weak in the theme.

These judgments are understandable and probably not grossly unfair. Nevertheless, I believe that they are based on the worst the *Report* has to offer and not on the best. At its worst, the *Report* is full of exaggerations, errors, confusions, conflicting statements, and careless writing. These no doubt represent, as the critics suggest, the "real" *Report.* But, if one attributes the exaggerations to a desire to escape the conventional, the conflicting and hazy statements to the task of achieving unanimity among diverse committee members, the confusions and errors to deadlines—if, in short, one excuses the weaknesses—and chooses the very best from its pages, the *Report* has much to recommend it.

In this idealized form, the *Report* presents a pioneering analysis of Britain's financial system, in which the monetary system and money are considered as only part of a complex, but integrated, structure of financial institutions, assets, and markets, and in which monetary policy, debt management, and fiscal policy are treated as coordinating techniques of a general financial policy aimed at regulating spending through this financial structure. In the underlying theme of the *Report,* all issuers of financial assets are relevant to financial policy; the private sectors issuing their debts and equities; the Treasury issuing various forms of government securities; the monetary system creating money and other claims; and nonbank intermediaries creating liquid claims. The idealized *Report* sees the level and structure of interest rates, which are the immediate targets of financial policy, determined partly by the whole range of financial assets—the level by the relation of liquid assets, including money, to holdings of financial and physical assets, and the structure to the composition of financial assets and demands for these components, with expectations playing their role in both cases. It sees money as only one asset among many, banks as only one type of institution among many, and the con-

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trol of money as only one aspect of an over-all financial policy. This is the *Report* at its best.

The Committee on the Working of the Monetary System, known as the Radcliffe Committee, was appointed by the Chancellor of the Exchequer in May 1957 "to inquire into the working of [Britain's] monetary and credit system, and to make recommendations." The Committee was composed of the chairman, Lord Radcliffe, two businessmen, two bankers, two trade union leaders, and two academic economists, the latter being Professors A. K. Cairncross and R. S. Sayers. During the fifty-nine days of hearings, scattered from July 1957 to April 1959, evidence was obtained from the Treasury, the Bank of England, clearing banks and other financial institutions, dozens of trade associations, business leaders, and many economists. In addition, the Committee received memoranda from many of these witnesses and from others, including the National Institute of Economic and Social Research, the Central Statistical Office, and, inevitably, a few monetary cranks.

The unanimous *Report* of the Committee was published in August 1959. This was followed in March 1960 by four volumes of evidence.[1] The first of these, *Minutes of Evidence*, is a huge, 980-page, double-column tome of the hearings. The other three volumes—*Memoranda 1, 2* and *3*—contain 142 papers submitted by various groups and individuals. As a rough guess, the *Report* and four volumes of evidence are packed with upwards of 3½ million words, a fact that I must note because it is possible that I have inadvertently slid over a hundred thousand or so here or there, and by so doing have done less than full justice either to the Committee or to some of its contributors.

I begin with a brief outline of the Committee's principal views on monetary theory and policy. I shall then discuss these views, in about the same order, supplementing the discussion with material from the oral and written evidence.[2] Since this procedure misses three areas that occupied much of the Committee's time—international finance, the status of the Bank of England, and financial statistics—I touch upon these briefly at the end.

I. *The Report's Monetary Theory and Policy: An Outline*

The following appear to qualify as the Committee's dominant views on monetary theory and policy:

1. The money supply has been largely uncontrolled during the postwar period; neither the banks' cash ratio nor their liquidity ratio has placed an effective upper limit on monetary growth.

2. But the money supply is of no great concern. First, it is incidental to the level of interest rates. These rates, though affected by liquidity, are determined largely by the expectations and confidence of the public, which in

[1] The five volumes of the Committee on the Working of the Monetary System are: (1) *Report* (London 1959), pp. viii, 375, 15s; (2) *Minutes of Evidence* (London 1960), pp. ix, 980, £7; (3) *Memoranda 1* (London 1960), pp. vi, 308, £2 10s; (4) *Memoranda 2* (London 1960), pp. vi, 227, £2; (5) *Memoranda 3* (London 1960), pp. vi, 251, £2.

[2] Unless otherwise indicated, footnote references have the following meanings: R, 000 refers to the *Report* and its paragraph number; MinE, 000 refers to *Minutes of Evidence* and the number of the question-answer; M-1, 000 refers to the first volume of *Memoranda* and its page number; and similarly for M-2, 000 and M-3, 000.

turn are determined by what the monetary authorities say. Second, there is virtually no direct relationship between the money supply and spending.

3. Interest rates, in and of themselves, have had little or no direct effect on spending decisions.

4. While spending is not directly affected by interest rates, it is affected by liquidity, which is composed of the money supply and the money people can get hold of. The private sector's liquidity is increased by the lending of commercial banks and other financial intermediaries, because such lending increases the supply of loanable funds ("the money people can get hold of"), and the growth of liquidity stimulates spending. The important thing about financial institutions is not the liquid liabilities (monetary or otherwise) they create but the lending they do—the assets they purchase.

5. The lending of all financial institutions can be indirectly controlled through changes in the level and structure of interest rates. A rise in interest rates will slow down their lending by imposing capital losses on their security holdings. Thus, while a rise in interest rates has little direct effect on spending, it depresses spending indirectly by reducing the lending of financial institutions and so the public's liquidity.

6. This means that the "centre-piece" of monetary action is the level and structure of interest rates, and not the money supply. The structure of rates can best be influenced by debt management policy working on the composition of the national debt. Since, however, it is not a good thing to have highly fluctuating interest rates, and since the lending of financial institutions in ordinary times should not be directly controlled, more emphasis should be placed on fiscal policy as a short-run stabilizer, leaving monetary policy to set the tone of longer-run developments.

7. But, during emergency situations, when runaway inflation threatens, monetary policy should be used vigorously. In these cases, lending should be restricted directly, by controls on commercial-bank lending, perhaps by the extension of such controls to other intermediaries, by consumer credit regulations, and by restrictions on issues of long-term securities.

II. *Control of the Money Supply*

A. *Definition of Money*

The Committee regards the money supply as consisting of notes[3] (but evidently not coins!) outside of banks plus net deposits, the latter being liabilities of London clearing banks and of Scottish and Northern Irish banks.[4] However, nowhere in the *Report*—and, indeed, nowhere in the *Minutes of Evidence* or the three volumes of *Memoranda*—is there an extended series of the money supply in accordance with this definition.[5] Bank deposits com-

[3] Most of them issued by the Bank of England but some by Scottish and Northern Irish banks. R, 347, 349.

[4] R, 388, 478.

[5] Partial series, which are either inconsistent with one another, refer to different things, or are for different dates, are scattered around like Easter eggs. The *Report* shows total deposits of London clearing banks (R, 134) and of Scottish banks (R, 149), but only for 1958; net personal deposits in London clearing banks for four postwar years (R, 478);

prise not only current accounts, which earn no interest and are repayable on demand, but also deposit accounts, which are similar to our time deposits, not subject to check, and hence, strictly speaking, not used as money by the public.[6] The Committee recognizes that deposit accounts are not quite the same as current accounts, but nevertheless they are counted as money. The reason seems to be that they are easily exchangeable for money.[7] The inclusion of these accounts raises the question of why other highly liquid claims, such as deposits in acceptance houses, in hire-purchase companies, in Trustee Savings Banks, and so on, are excluded.[8] While the Committee does lay down an explicit definition of money, at other times it goes out of its way to "fuzz up" the concept, by placing the supply of money in quotation marks, followed by "however that is defined," "whatever that may be made to mean," and similar derogations.[9]

B. *Cash and Liquidity Ratios of Banks*

The Bank of England normally attempts to control the volume of bank deposits in the traditional way—by controlling the supply of certain bank assets which are held, by convention and not by statute, in fairly constant proportions to deposit liabilities. The conventional cash ratio is 8 per cent, with cash consisting of vault cash and deposits at the Bank of England. Deposit expansion is also limited potentially by a conventional 30 per cent liquidity ratio, where liquid assets for this purpose include cash, Treasury bills, call loans, and commercial bills.[10] There has been a spirited debate in recent years concerning which ratio, if either, is the effective one.

To reduce bank deposits, the Bank of England can decrease the banks' cash and thus their liquid assets by selling securities in the market. To regain liquidity, the banks can sell short-dated bonds, which do not count as liquid assets, to the discount houses, the latter financing the bonds by borrowing at call from the banks, with call loans counting as liquid assets. Or the banks may sell bonds and buy Treasury bills or commercial bills. It has been easy enough for banks to get liquid assets, but none of these operations replenishes their cash. For this purpose the banks must borrow from (or sell something to) the central bank. In the United States, borrowing is done directly; in Britain it is done indirectly through the discount houses. These houses hold

and a chart of the ratio of the money supply to national income for 1946-58 (R, 478). Elsewhere, data are presented for currency with the public and net deposits of clearing banks (M-3, 69), note circulation and clearing bank deposits (M-3, 184), in chart form the ratio of the money supply (currency with the public and net deposits of clearing banks) to GNP (M-3, 102), note issues and deposits of all reporting banks and acceptance houses, with intergroup items not omitted (M-2, 202-03), and in chart form the ratio of net bank deposits to national income (MinE, 10433).

[6] R, 128-31.

[7] R, 131. The Scottish banks appear to treat their deposit accounts somewhat differently. See, R, 153.

[8] For example, see MinE, 8033-35 and 8132 for evidence that deposits in Trustee Savings Banks are more and more being used as current accounts.

[9] R, 125, 504; also 395, 523.

[10] R, 143-48, 351; M-1, 9-10

Treasury bills, short-dated government bonds, and commercial bills, and they borrow at call from London clearing banks and other banks. In addition, they and not the clearing banks are permitted to borrow from the Bank of England. When the clearing banks lose cash, therefore, they may call some loans from the discount houses. Under pressure, these houses may then either sell Treasury bills to or borrow from the Bank of England, and in these ways restore to the clearing banks the cash initially lost.[11] But, as the Bank explained to the Committee:

> It should be noted that the creation of a cash shortage is not without effect. For, particularly, if it is such as to compel borrowing from the Bank of England, which is relatively expensive to the Discount Houses, they will try to reduce such borrowing so far as possible by raising their bids for money [that is, selling bills or short bonds], thus causing short-term interest rates to rise.[12]

This was further explained, in response to a question from the Committee, as follows:

> If we wanted to raise interest rates, then we would give less or possibly no help in the ordinary way [by purchasing bills from the discount houses], and we would say: "If you want cash you must come to the Discount Office for it." And moreover we could if need be so arrange things that the market needed a great deal of cash. . . .[13]

This is the familiar process by which open-market sales of a central bank reduce the "cash" of commercial banks and force them to reduce earning assets and the money supply, raising market rates of interest. Commercial banks can get more cash from the central bank any time they want to, but they presumably have a demand schedule for cash that is negatively related to its price. Hence, if the central bank raises the price, the banks will demand less cash, and so less earning assets and less monetary liabilities.

The Committee seems to have misunderstood this evidence, taking it to mean that, since banks could get more cash at any time, the cash ratio was therefore ineffective in limiting the money supply. This confusion appeared several months later when the Committee was again questioning spokesmen for the Bank. The latter stated that during times of inflationary pressure it would be better for the government to finance a deficit by selling Treasury bills to the market than by borrowing from the central bank (by Ways and Means advances), inasmuch as, though either measure could increase the liquid assets of banks, the latter would raise their cash base. This prompted the following exchange:[14]

[11] R, 175, 355-58; M-1, 9-10; MinE, 183-89, 1568-96.

[12] M-1, 10.

[13] MinE, 98. For a similar statement, see MinE, 421, which the Committee quotes in R, 360.

[14] In this and subsequent exchanges quoted here, the first part of each paragraph is a question from some member of the Committee, and the second part, after the dash, is the answer by one of the witnesses. I have not thought it necessary most of the time to identify the interrogators and respondents.

I understood their policy of increasing deposit liabilities would be determined by their total of liquid assets, not the amount of cash? —— The two things come out to the same, in the sense that the total of liquid assets in either case is affected in the same way; but the composition of their liquid assets would be different. With the Bank of England making Ways and Means Advances, their cash would be inflated. It would be inflated to a point beyond the 8 per cent, they require, and to that extent they would be looking round for earning assets. . . .

Is not this a different doctrine from what we have been told hitherto? ——I do not think so. . . .

We are now back on the question of the whole control of the volume of bank deposits; we have been taught hitherto that it is the liquid assets ratio that matters?——Surely we have said there are two aspects of this control. . . .[15]

Passing over this and other evidence, the Committee in its *Report* states that the cash ratio was ineffective.[16] Nevertheless, at other times, the Committee seems to recognize that this is incorrect, for it is stated, in substance, that the cash ratio would be ineffective only if the Bank of England set out to stabilize the Bill rate (which it generally has not done); otherwise, it would be effective.[17] But it is the former view that carries the day. In the Committee's eyes, the more effective ratio is the liquidity ratio, though it is stressed that banks have found it comparatively easy to evade this, through the mechanism described above, and that it has been thirty years since bank lending has been restrained by liquidity considerations.[18] In the end, one gathers that the Committee believes that the money supply has been largely uncontrolled.

These views are not easily reconciled with the fact that interest rates have increased sharply during the last decade in Britain, unless of course it is held that the money supply has nothing to do with the level of interest rates, a proposition I turn to in a moment. The banks have been subject, it is true, to requests to hold the line on or to reduce their advances (loans and overdrafts), but these restrictions have been short-lived, and in any case the banks could always purchase other earning assets and so increase their deposit liabilities.[19] In view of all the evidence, it seems more reasonable to conclude

[15] MinE, 2262-70. It should be noted, though, that at times even the Bank argued that the cash ratio was largely ineffective because it had to act as a lender of last resort to the discount market. (M-1, 38). However, it was brought out in testimony that this meant only that the Bank, as lender of last resort, would give the market, *at a price*, all the cash it wanted; it did not mean that the Bank, as lender of last resort, relinquished all control over interest rates (MinE, 94-101).

[16] R, 120, 376, 430, 583. In fact, in direct opposition to the above evidence, the *Report* says: "But now that the credit-creating capacity of the banks is limited by the 30 per cent liquid assets' convention, an increase in cash precisely balanced by a decrease in the Treasury Bill issue has become irrelevant to the credit-creating power of the banks" (R, 167).

[17] R, 376. See also R, 121, 355, 357.

[18] R, 120, 167, 175, 505-7. For further discussion of the weaknesses of the liquidity ratio, see MinE, 183-89, 1568-96, 1654-58, 1909-13, with supplementary notes to 1909 on pp. 952-54, and 3738-65.

[19] R, 411, 417, 422; MinE, 2677 ff., 3265-72, 3523-3645; and M-1, 40. However, if the

that there has been more or less effective control of the money supply, at least since 1951, and that this control has probably operated primarily through the cash base of the banks, as described by Bank officials.

III. *Interest Rate Determination*

At one point in its *Report*, the Committee presents an admirable account of the determination of the level of interest rates. After extolling the role of liquidity in economic analysis, the statement continues:

> We would nevertheless emphasize that the amount of money . . . is of considerable significance. The other classes of liquid assets . . . are inferior, in convenience to the holders, and this inferiority has to be compensated by the payment of interest. If there is less money to go round, in relation to the other assets (both physical and financial), it will be held only by people willing to make a greater sacrifice in order to hold it: that is to say, rates of interest will rise. But they will not, unaided, rise by much, because in a highly developed financial system . . . there are many highly liquid assets which are close substitutes for money, as good to hold and only inferior when the actual moment for a payment arrives . . . (i.e., the more efficient the financial structure, the more can the velocity of circulation be stretched without serious inconvenience being caused).[20]

Unfortunately, this statement is in sharp conflict with what appears to be the Committee's principal view of this subject. While it can be reasonably argued that the *Report* does not in fact present a theory of interest rate determination—or, perhaps better, that it obliquely presents several—it is fair to say, I think, that the Committee looks upon "expectations and confidence" of the public as the chief determinant of rate levels—though lip service is paid now and then to the role of liquidity. And these expectations are greatly affected by what the authorities say, by the public's interpretation of their mood. That is, expectations are molded by the "faces" made by the authorities; presumably, a squinty-eyed look might raise interest rates, a vapid stare maintain them, and an ebullient expression lower them. When this theory takes over, the authorities never change interest rates by operating on the money supply or on liquidity generally; rather: "The authorities must seek . . . to influence the general liquidity situation by operating on rates of interest."[21] It is even stated that the money supply is incidental to interest rate policy.[22] It is for this reason that the Committee can properly hold the view that the money supply has been largely uncontrolled in the face of sharp changes in levels of interest rates; for after all money has little or nothing to do with interest rates.[23]

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control over bank advances forces the banks to purchase other earning assets that are more liquid than those they would have purchased, a given money supply is likely to be associated with a higher interest rate structure, other things the same.

[20] R, 392 and footnote.

[21] R, 504; also 385, 397 ff.

[22] R, 397. This is also suggested in R, 394.

[23] In other words, while there may be a liquidity-preference schedule, drawn against

The theory that interest rates are determined by words and faces is indicated repeatedly in the *Minutes of Evidence*.[24] As an example, consider this exchange with Winfield Riefler, then assistant to the chairman of the Board of Governors of the Federal Reserve System:

> Then is not much the most important thing you do not the buying and selling of Bills, not the raising or lowering of [discount] rates, but what you *say?*——No.
>
> Why do you have to operate in the market as well as telling the market what you think about things?——Our operations in the market actually determine the funds available.[25]

This evidently left the Committee incredulous. Though there are some indications that it was not willing to accept wholeheartedly the dominance of expectations in interest rate determination,[26] the truth is that this factor, by elimination, becomes the only solid explanation of rate movements in the *Report*, a theory which suggests that bond markets in Britain are peopled by a lot of nervous wrecks. The Committee was also influenced in this view by R. F. Kahn,[27] to whom I shall return later.

The Committee landed in this position despite the fact that F. W. Paish and others presented data to show the very close postwar relationship between the ratio of money to national income and bond rates,[28] and despite Paish's excellent testimony about this relationship. Here is a sample of it:

> During the period up to 1956 during which there was this tremendous drop in the ratio of bank deposits to national income, was there not considerable liquidity in business? —— If there is so much liquidity, why are firms willing to pay very high rates of interest for raising long-term loans on the London market?
>
> They may expect prices to rise? —— It is liquidity in relation to what they want to do. I would say that the long-term rate of interest is the inverse of liquidity.
>
> Would this mean that you would feel that if you knew the ratio of bank deposits to net national money income you could predict what the rate of interest would be? —— So long as the conditions remain, I would say almost exactly.
>
> The point you are putting to us is that there is an inverse relationship

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interest rates, the schedule is subject to such wide shifts, as expectations change, that interest rates are in fact little influenced by the amount of liquidity in the system.

[24] I admit that it is a tricky business to impute views to the Committee from questions asked and statements made by it during examination of witnesses. But I have done this only when such evidence seems consistent with the final views of the Committee as stated in its *Report*.

[25] MinE, 9760-61; also 10220, 10236. My emphasis.

[26] See, for example, R, 563, as modified by 565. Note also the Bank's stress on confidence as the prime determinant of interest rates in MinE, 2398-99, qualified slightly in supplementary notes of the Bank, pp. 955-56.

[27] R, 395. For Kahn's opinions on this, see M-3, 144.

[28] MinE, 10433; M-3, 102, 184-85.

between the liquidity of the system and the rate of interest, liquidity being defined not just in the terms of the money supply, but to include near-money? —— I would put it in terms of the money supply. One would expect the amount of near-money to affect the shape of the curve. . . .

The issue is whether the relationship is sufficiently close and the lags sufficiently limited to allow of operational application? —— I would say that in the short run one can get it down to pure expectations if one can persuade people that long term rates are going to fall and that they will have a heavy capital appreciation. There could be very marked temporary shifts on those expectations; but if the authorities wanted to stabilize the long term rate round about 4½ per cent., they would have to allow the ratio of bank deposits to national income to rise to more nearly 40 per cent than 35 per cent.[29]

And in response to an additional question about the effects of near-money, he answered:

I would say that whereas they could get a given rate of interest with a 40 per cent. ratio of bank deposits to national income if there was not very much near-money in the system, they might need 35 per cent. to get the same rate if there was a lot of near-money in the system. They would have to set off the increased liquidity due to large holdings of near-money by having less real money, in order to get the same effect on total liquidity.[30]

Excellent memoranda and testimony were also submitted on this question by many others. Altogether, there seems to be an impressive body of evidence to support the view that the money-income ratio, modified by the presence of other liquid assets, and within the context of "real" variables, was the principal determinant of the level of interest rates in Britain during the postwar period.[31] But, for some reason, the Committee chose to ignore this evidence.[32]

IV. *The Direct Effects of Interest Rates on Spending*

The Committee adopts a "three-gears" view of interest rates. At any time, people believe that interest rates are either in low, middle, or high gear. Any play of rates within a given gear is not likely to have much, if any, direct effect on spending. If people "are to be shaken into some change of course, the gear must be changed."[33] A rise in the Bank Rate from 4½ per cent to 7 per cent, for example, would generally be regarded as a shift from middle to high gear.

Nevertheless, though such upward shifts have occurred in postwar Britain, and so might have produced downward pressure on spending through the "interest rate effect," the Committee states that it found very little evidence that

[29] MinE, 10431-32, 10434, 10436, 10438.

[30] MinE, 10441. See also MinE, 10443-53 for further discussion of these points.

[31] See M-3, 66, 179, 183-85, 85, 113, 146-48; and MinE, 10425-512. Outstanding memoranda on this subject were produced by James V. Morgan and J. C. R. Dow.

[32] For one flagrant example, see R, 570, where the Committee presents three reasons for the rise in bond rates without once mentioning the money supply or national income.

[33] R, 442-43.

this has in fact happened.[34] According to the Committee, it found no evidence that higher interest rates, in and of themselves, reduced consumption; there was practically no indication that interest rates were important to large firms with respect to investment in either inventories or fixed capital; expenditures of the nationalized industries were also largely impervious to changes in interest rates; the same was true for local authorities' expenditures; and spokesmen for the smaller firms treated the interest rate effect with general skepticism.[35] "It has become clear that, as the system works at present, changes in rates of interest only very exceptionally have direct effects on the level of demand. . . ."[36] If we accept these statements, they would seem to dispose once and for all of the subject, as it relates to postwar Britain up to 1959.

But, from other material presented to the Committee by witnesses and others,[37] it is doubtful that the statements can be accepted in their present extreme form. When this evidence is compared with the conclusions of the Committee, it is hard to escape the feeling that its final appraisal should have been more guarded.

There is, first of all, the survey made by the Federation of British Industries of its manufacturing members with respect to the impact of higher interest rates in early 1955 on their investment decisions. Among the questions, was: "Was the rise in Bank Rate from 3 to 4½% during January and February, 1955 a *major* factor in taking your business decisions?" (Their italics.) Almost 12 per cent (179) of the firms answering (1526) said that it was. Of these 179 firms, 40 per cent were in the engineering, shipbuilding, electrical, and textile industries. The rest were found among more than a dozen other industries. These firms reported deferment or reduction of investment projects, or reduction or deferment of inventory purchases, or other action.[38]

The Committee, in commenting on this survey in its *Report*, states:

> The response rate . . . was relatively low, and it may well be that answers came for the most part from those who had some positive reaction to report . . . in discussions with us representatives of the Federation were not confident that these figures could be regarded as firm enough to be the basis for general conclusions.[39]

The first part of this statement suggests that those who did not answer the survey may have had a lower ratio of yes-to-no answers than those who did. This may well be: but it should be noted that the questionnaire contained dozens of questions on other subjects, so that the nonrespondents could have had many other reasons for not participating.

As to the second part of the statement, I can find nothing in the *Minutes*

[34] R, 386.

[35] R, 450-51, 489, 495.

[36] R, 487. Sayers kept pressing the view on witnesses that an increase in interest rates will raise actual investment in the short run. His theory was that higher rates will lower consumption and so leave more room for investment, given aggregate demand. No witness accepted this, and it does not show up in the *Report*. See, *e.g.*, MinE, 4189-92 5656-61.

[37] Some of this material appears in the *Report*, R, 452-53.

[38] M-2, 118-22.

[39] R, 453.

of Evidence to support it. What I do find is a continual "hounding" of the witnesses, a barrage of counterarguments and suggestions from the Committee, until finally the witnesses wilt under extreme pressure and state that "it might not be so." First of all, the Committee made it quite clear to the witnesses that it considered the figure of 179 out of 1595 "astonishingly high" and "most surprising," especially in view of the fact that the firms at the time probably anticipated inflation. The witnesses, after some hesitation, finally agreed that the figure might indeed be surprising. The Committee then asked them whether such expectations of inflation might not swamp the effect of higher rates [yes], whether other measures that accompany the higher rates might not be responsible for reduced spending [very likely], whether the interest rate can be isolated from other influences [yes], and whether the interest rate is really a factor taken into account before investment is made [not a principal factor]. Having prodded the witnesses into saying that interest rates are relatively unimportant, the Committee next asked them how, in view of that, the interest rate came to be a major factor with 179 firms. The answer and the Committee's response to it follow:

> [First witness:] I am becoming nervous about how well the question was understood by these 179 people. [Second witness:] I do not think the 179 merely took account of the rise in interest rates alone; they took it in the context of the other measures. It is not explicit in the answers. [First witness:] We could go back to these 179 firms, and cross question them about this, and perhaps get more information.
>
> It is rather vital to our discussions. We have had so many people tell us over so many years that, at any rate in the manufacturing industries, the rate of interest hardly affects these decisions at all, and one has come to think that whatever else Bank Rate changes can do they cannot do anything much about investment in the manufacturing industries. —— [Answer not relevant.]

And, finally, after the witnesses agreed that the factor of uncertainty in investment projects diminished the importance of interest rates—still another observation having nothing directly to do with the survey results—we find:

> That is exactly what the university lecturer says when he is lecturing. He is describing the position correctly when he says that? —— In my limited experience, yes.
>
> You will see why, after having had years of that, one finds these figures so surprising? —— It is a complex of factors, and this expectation of inflation has, I am sure, been a very prominent consideration in that complex.
>
> My experience as a partner in an issuing house is relevant [to what has just been said]. I cannot remember a case where a company has come to my firm, and asked what the cost of money would be. —— [Answer not relevant.]
>
> Is there not another aspect to this? A great deal of capital expenditure so classified is in fact a total of a very large number of small decisions taken throughout the period; in relation to those decisions all sorts of practical considerations of the market such as have been mentioned are

really far more relevant and major considerations than the cost of money? —— Yes.[40]

Well, the theory of the university lecturer won out over the facts. "In discussions with us," the Committee said—and I come back to this without further comment—"representatives of the Federation were not confident that these figures could be regarded as firm enough to be the basis for general conclusions."

Another survey in March 1958 was taken by The Association of British Chambers of Commerce, inquiring about the effects of the credit squeeze after September 1957 on a wide range of companies. 16,000 questionnaires were sent out, but there were only 3404 usable responses. Of those stating that they had, since September 1957, experienced reductions in their turnover or in investment programs, only 4 per cent said they were due to the higher cost of borrowing, though 20 per cent attributed them to that plus tight money and restrictions of bank credit. And 30 per cent of those firms said that they had taken steps to reduce or pay off their bank borrowing because of increased costs.[41] After noting this, the Committee throws doubt on the results in this way:

> . . . but it may well be that the dramatic rise in cost, which certainly attracted much attention, was being blamed for reductions many of which were in fact dictated much more by expectations of a decline in the level of activity than by the rise in the rate of interest.[42]

Its concluding remarks about these two surveys are also interesting:

> The results of these questionnaires add up to substantial evidence that a proportion, big enough to be relevant to policy, of business firms were vaguely discomforted by the changes in monetary conditions in 1955-57, and especially in September 1957; but we have not found sufficient evidence to justify a conclusion that in the conditions of the 1950s the rise in interest rates would by itself have directly provoked a worthwhile curtailment of demand.[43]

Aside from what is "worthwhile," the last part of this statement is necessarily true because the surveys did not seek to determine the amount of reduction in demand due to higher interest rates but only the number of firms reporting such a reduction.

A third survey was carried out, in October 1957, by the Birmingham Chamber of Commerce; questionnaires were sent to 3,400 member firms, and 610 responded. 185 firms reported that they had postponed or cancelled plans after 1955 for new factory or office buildings, extensions to existing buildings, replacement of machinery or equipment, or orders for new machinery or equipment. Of these, almost 40 per cent attributed such reductions to the increase in interest rates on borrowed capital.[44]

[40] This is part of the full exchange found in MinE, 5566-5617.

[41] M-2, 88-96; R, 453; MinE, 11119 (and footnote).

[42] R, 453.

[43] R, 453.

[44] M-2, 87-88.

During the hearings with the representatives of the Association of British Chambers of Commerce, this was brushed aside rapidly, in the following way:

> In the Birmingham inquiry about 40 per cent. of those who postponed or cancelled plans attributed this to an increase in interest rates. That is a very high proportion. Did it not surprise you? —— I have no explanation of the results. It may be something that was peculiar to Birmingham. I wonder whether this may have had something to do with the recession in the motor industry.
>
> If we leave the Birmingham inquiry aside for the moment, and concentrate on your own much more elaborate inquiry. . . .[45]

As it turned out, the "moment" proved to be of infinite duration.

That is by no means all of the evidence submitted to the Committee that ran contrary to its general conclusions. Not all of the following evidence is clear-cut; in fact, much of it is at best sketchy, but all of it stood up under sharp questioning.

For example, representatives of the British Engineers' Association stated that higher interest rates increased their costs and so hurt their business;[46] those of the Country Landowners' Association testified that higher interest rates had cut back farm improvement projects substantially;[47] The Scottish Landowners' Federation had essentially the same story, claiming that capital expenditures in agriculture and forestry had been reduced by higher interest rates and would almost certainly be stimulated if credit became easier;[48] for this reason capital expenditures were also reduced by wholesalers, and their demand for inventories was curtailed;[49] it was the opinion of representatives of The Association of Investment Trusts that higher rates had cut back business spending all along the line;[50] a furniture dealer said that the rise in rates in September 1957 caused him to reduce capital expenditures, and that an increase in the Bank Rate "does come very much into our planning for the present and for the future";[51] a dealer in wine and spirits offered similar testimony;[52] witnesses for the Association of Municipal Corporations knew of several instances, involving water projects, housing programs, and so on, where expenditures were cut back because of higher interest rates;[53] a small amount of local authority's expenditures was said to be affected by a change in rates;[54] there is some evidence, though not much, that rubber merchants reduced inventories because of higher rates;[55] a survey carried out by the Council of Scottish Chambers of Commerce found that one-third of those firms replying stated that they had taken steps after September 1957 to reduce bank loans because of the higher cost;[56] the Committee heard that the high cost of money was a factor in reducing inventories of automobile dealers;[57] it also heard that retail chemists reduced or postponed capital expenditures and inventory purchases partly because of higher borrowing costs, though this was qualified in response to questioning;[58] a representative of the North of Scot-

[45] MinE, 11169-70. [46] MinE, 6266-69.

[47] MinE, 6427-29. [48] MinE, 6490-93, 6499. [49] MinE, 6734-47. [50] MinE, 7568-79.

[51] MinE, 8149, 8155-67, 8171-75, 8178. [52] MinE, 8188, 8206. [53] MinE, 8263-66, 8276. But see 8279 where this view seems to be modified. [54] MinE, 8544. [55] MinE, 8583-85.

[56] MinE, 8918. [57] MinE, 11360-62, 11414. [58] MinE, 11605, 11618-28.

land Hydro-Electric Board stated that higher rates undoubtedly slowed down their program for distributing electricity;[59] the building of an hotel was deferred partly because of higher borrowing costs;[60] evidence was put forth that some wholesale tobacconists, timber people, and others reduced inventories because rates went up;[61] two bankers stated that the higher rates after September 1957 caused some businesses to reduce overdrafts;[62] the same was reported by other bankers, who further claimed that merchants, and to a lesser extent, industrial concerns reduced inventories and that some capital expenditures were curtailed for interest-rate considerations, though this view was modified under questioning;[63] London clearing bankers, too, reported that bank loans probably fell after September 1957 due to higher borrowing rates.[64] The Committee also had the opinions of several economists that a change in interest rates was effective in altering spending throughout the economy;[65] and the Bank of England concurred with this.[66]

It is far from certain, of course, what all this adds up to, especially since an equally impressive list could be produced for the other side. But, as a minimum, it would seem that the Committee's extreme conclusions are presumptuous in view of the evidence it heard.

V. *Liquidity and Financial Institutions*

As I have noted, the Committee all but eliminates the money supply as a factor in the determination of interest rate levels; it believes that changes in these rates have had little direct effect on spending; and it does not think that there is any direct, close connection between the money supply and spending. But, while money is shoved out of the house through the front door, for all to see, it does make its reappearance surreptitiously through the back as a part of general liquidity: and the most important source of liquidity is the large group of financial institutions.[67]

This is the reason the Committee devotes much space in its *Report* not only to the monetary system but also to the large number of nonbank financial intermediaries. The more important of these intermediaries are the discount houses, hire-purchase companies, insurance companies, superannuation and pension funds, Post Office Savings Bank, building societies, and investment trusts. At the end of 1958 the assets of this group exceeded those of the monetary system by about 60 per cent.[68]

It is no simple matter to discover in the *Report* by what process the intermediaries alter liquidity, for the Committee shifts around from one point of view to another and never does get down to definitions. But its principal view seems to be that the public's liquidity is composed not only of the

[59] MinE, 11726.

[60] MinE, 12287. [61] MinE, 12992-96. [62] MinE, 12952-55.

[63] MinE, 4976-86. [64] MinE, 3617, 3646. [65] M-3, 178-82, 182-88, 95, 213. [66] M-1, 35-38.

[67] R, 125, 389.

[68] The monetary system is composed of the Bank of England, the London clearing banks, and the Scottish and Northern Irish banks. The assets of the separate financial institutions for several postwar years are given in the memorandum of the Central Statistical Office (M-1, 130-41), but the most recent data are in the *Report*, Table 20, and referred to in R, 313.

money supply but of the amount of money it can get hold of; at one point, the matter is put in even vaguer terms when it is stated that liquidity in the broad sense depends on "the amount of money people *think* they can get hold of."[69] Since people can get hold of money from financial institutions, liquidity is increased when such additional borrowing sources become available: the greater the number of potential lenders, especially institutional lenders, the greater is the public's potential liquidity, because it is then easier to raise funds.[70]

Put somewhat differently, the notion is that the proliferation and growth of financial intermediaries increase the demand for "bonds," which "makes money more available" and stimulates spending for current output, even though the banking system is tightly controlled. With one possible exception, nowhere in the *Report* is there a statement as explicit as that, and nowhere does the Committee attempt to explain the process just described.[71] The reader is simply left with the thought that if a new intermediary comes along, or if an old one grows, the aggregate demand for "bonds" is somehow increased—there is an increase in the supply of loanable funds, and more money is made available to potential spenders.

The Committee fails to note that the extent to which this is true depends on whether the growth of intermediaries reduces the public's demand for money—and so money becomes vital to the analysis. This may be illustrated as follows. Assume that there are three financial assets—bonds, money, and savings deposits—which are liabilities of the public, the monetary system, and nonbank intermediaries, respectively. Assume, further, for simplification, that the assets of all financial institutions consist only of bonds. Then, in the usual definition, the supply of loanable funds is defined:

$$(1)\ \text{Supply of Loanable Funds} = \begin{cases} \text{Planned saving by public} \\ + \text{Increase in stock of money} \\ - \text{Increase in demand for money (hoarding).} \end{cases}$$

To simplify further, assume that saving and investment are done by different groups and that savers do not repay debts. Then planned saving is equal to the public's increase in demand for bonds, money, and savings deposits; the public's increase in demand for savings deposits is equal to nonbank intermediaries' increase in demand for bonds; and the increase in the stock of money is equal to the monetary system's increase in demand for bonds. Hence,

$$(2)\ \text{Supply of Loanable Funds} = \begin{cases} \text{Increase in demand for bonds by the public,} \\ \text{nonbank intermediaries, and the monetary sys-} \\ \text{tem.} \end{cases}$$

It may be seen, then, that an increase in demand for bonds by nonbank intermediaries will increase the supply of loanable funds only if it is not accompanied by an equivalent or greater decrease in demand for bonds by the public and the monetary system. Suppose the public increases its demand for

[69] R, 390. My italics.

[70] R, 316, 389, 390.

[71] The possible exception is R, 392.

savings deposits, enabling nonbank intermediaries to increase their demand for bonds. Given the public's propensity to save, this means that there is a reduction in the public's demand for either money or bonds. If bonds, then the reduction in demand for bonds by the public is exactly offset by the increase in demand for bonds by nonbank intermediaries, and so the supply of loanable funds remains the same. On the other hand, if the shift is away from money, then the public does not reduce its demand for bonds, and neither does the monetary system reduce its demand for bonds (because a decrease in demand for bank liabilities will not alter the monetary system's outstanding liabilities and so will not change its bond assets). Hence, in this case, there is a net increase in the supply of loanable funds.

It is possible, therefore, that the purchase of bonds (lending) by nonbank intermediaries will not increase the supply of loanable funds. If it does not, then the public's increase in demand for money and savings deposits is matched exactly by the increase in the stock of these assets. There are more "liquid assets" in the economy, but there is also an equivalent increase in demand for them. The amount of funds made available to the public, at given interest rates and other terms of lending, is exactly the same as before; the growth of nonbank intermediaries has not changed the over-all situation.

It follows that the extent to which the growth of nonbank intermediaries will increase the supply of loanable funds (or "liquidity" in the Committee's terms, i.e., the excess stock of liquid assets) depends on the degree of substitutability between savings deposits and money, so that an answer to this question requires an analysis of the types of claims issued by the intermediaries. By concentrating on the asset side of the intermediaries' balance sheet, and neglecting the liability side, the Committee fails to come to grips with the problem. Its failure to compare what is being withdrawn from the market with what is being issued to the market—or, put another way, its failure to consider the demand for liquid assets as well as the supply of them —is at the heart of the difficulty. This one-sided view is reflected in many conclusions in the *Report:* that nonbank intermediaries are important only because they lend; that banks are important not because they create money but because they make loans; that it is not the money supply that should be controlled but bank advances; and so on.[72]

I cannot say that the Committee is wrong in stating that nonbank intermediaries have increased liquidity and the supply of loanable funds, and have

[72] On this last point, the basic difficulty with the Committee's approach stands out most clearly. It is stated that "regulation of the banks is required not because they are 'creators of money' but because they are the biggest lenders at the shortest (most liquid) end of the range of credit markets." (R, 504). That banks lend at the "most liquid end of the range of credit markets" is an argument against controlling them. The more the assets purchased by a financial institution resemble the liabilities it creates, the less need is there to control that institution. An institution, for instance, that purchased money and created money would not have to be controlled. (In another context, the Committee comes to this conclusion with respect to the note issues of Scottish banks which are backed by Bank of England notes.) Nor would one that purchased bonds and issued what the market considered to be identical bonds need to be controlled. Financial intermediaries become potentially more dangerous to the stability of the economy the more illiquid their assets are relative to their liabilities, given the rate and pattern of their growth.

done this in such a way as to exert a destabilizing influence on spending. These views may well be correct as applied to postwar Britain. The point is that the analysis leading to these conclusions is faulty, which not only leaves the reader unconvinced of them but leads the Committee down some wrong policy paths.

The Committee raises the question of whether the destabilizing activities of nonbank intermediaries should be controlled as the banks' are. On this it states:

> If we are right in believing that the level of total demand is influenced by the lending behaviour [the asset side again!] of an indefinitely wide range of financial institutions, and not just by the supply of money, it might be supposed that we should substitute for the traditional control of the supply of money a complex of controls of that wide range of financial institutions. Such a prospect would be unwelcome except as a last resort, not mainly because of its administrative burdens, but because the further growth of new financial institutions would allow the situation continually to slip from under the grip of the authorities.[73]

The *Report* later adds that:

> Any severely restrictive control of [bank] operations is certain, over a period of time, to be defeated by the development of rival institutions; during the interim, the community will have suffered loss by interference with the most efficient channels of lending. We therefore begin with some presumption against discriminatory control of banks, at any rate in ordinary times.[74]

In principle, then, there should be no discriminatory controls on banks in ordinary times; but only as a last resort should controls be imposed on other financial institutions; and whether they are imposed solely on banks or on a wider range of institutions they are bound to be undermined in the long run by the development of rival institutions. Leaving this somewhat ambiguous principle, which, not surprisingly, was endorsed unanimously by the Committee, it is stated that when you get right down to it the banks must be controlled.[75] However:

> If, in the light of future experience, it should appear desirable to reinforce the authorities' power by raising the minimum liquidity ratios of the banks appreciably above their present levels, the practicability of imposing comparable restraints on other groups of financial institutions should be considered.[76]

The question of controlling other financial institutions was raised with several witnesses, and other individuals expressed their opinions (or those of their organizations) in memoranda to the Committee. The most extensive discussion of this was carried on with M. H. de Kock, Governor of the South African Reserve Bank. Here is how a small part of it went:

[73] R, 394.

[74] R, 504.

[75] R, 505.

[76] R, 509. See also R, 510-11, 527.

Does it follow from [your] argument . . . that there is a danger that when you operate on the quantity of money the banks as particular financial institutions may be penalized, and that, if you are going to operate a credit squeeze, it should if possible operate widely over the whole field of finance and not narrowly on one set of institutions? —— Yes. Years ago, when commercial banks were the main financial intermediaries, the authorities could by contracting bank credit achieve all that they wanted to achieve. In the thirties we considered that that was all that was necessary. Since that time the development of these other financial institutions has been far more active than that of the banks, and they are encroaching more and more on the field of the banks in all sorts of little ways; and today the banks are hampered in their attempts to follow the requests of the central bank.[77]

Roughly similar views were expressed by T. Balogh and by representatives of the Trades Union Congress.[78] There were of course many dissenters, among whom was Winfield Riefler, who was torn between two opposing views: that nonbank intermediaries, when borrowing funds from the public, have no effect on monetary equilibrium; and that such intermediaries, when creating money substitutes, do upset monetary equilibrium.[79] He was not asked to reconcile these views. Others, including M. W. Holtrop, president of The Netherlands Bank, were on the same side. Holtrop stated that, inasmuch as nonbank intermediaries simply redistributed the community's savings, there was no reason to believe that they would create a monetary disturbance, but that his bank, nevertheless, collected data on their activities—why, he did not say.[80]

VI. *Debt Management and Monetary Policy*

The Committee's views up to this point seem to leave us in a box. The money supply has little to do with the determination of interest rates; changes in these rates have had little direct effect on spending; spending is instead influenced by liquidity, which is increased when financial institutions lend; but only as a last resort should lending by nonbank intermediaries be directly controlled, and only in emergency situations (as we shall see) should bank lending be so controlled. How is spending, then, to be regulated effectively by monetary techniques?

The answer is that the authorities can *indirectly* control the lending of financial intermediaries, and, hence, the liquidity and spending of the public, by changing the level and structure of interest rates. A rise in interest rates, for example, will slow down lending by these intermediaries to the private sector by imposing capital losses on their security holdings. Or, as Sayers put

[77] MinE, 9377. For de Kock's full views on this, see M-1, 289-90.

[78] M-3, 37; MinE, 10172-80.

[79] This testimony is in MinE, 9822-27. Riefler also dealt with this problem in his memorandum, in which he stated that nonbank intermediaries play a neutral role in the saving-investment process and that control of the money supply has pervasive influences throughout the whole financial structure. See M-1, 301.

[80] MinE, 11812-18. For similar, though milder, statements, see the memorandum of The Bank of Australia, M-1, 249, and the British Treasury's statements, MinE, 1607-11.

it during the hearings, "you insert the gelatine of uncertainty into their liquidity."[81] Moreover, if the national debt is lengthened at the same time, the liquid-asset base of commercial banks will be limited and thus their lending depressed.[82]

This brings debt management to the head of the class, because it is the principal means of affecting the level and structure of interest rates, and the rate structure should be considered "the centre-piece of monetary action." It is through this mechanism that regulation is exerted on the lending ability of banks and other financial intermediaries and on the public's ability to spend.[83] The authorities, in managing the debt, should not concentrate exclusively on short rates but should give a lead to the market on long rates as well; it is this structure of rates they should keep their eyes on and not "some notion" of the money supply.[84] In principle, during inflationary periods, the debt should be lengthened and interest rates raised; during deflation, the opposite policies should be followed.[85]

Thus the national debt, and not the money supply, becomes the focal point in the economy for the control of interest rates, and through these rates, for the control of institutional lending and thus the economy's liquidity and spending. In this set-up, banks are important because they are key lenders:

> It is the level of bank advances rather than the level of bank deposits that is the object of this special interest; the behaviour of bank deposits is of interest only because it has some bearing, along with other influences, on the behaviour of other lenders.[86]

In looking upon the national debt as the focal point of financial control, the Committee claims that it is following the evidence submitted to it by Kahn.[87] But, when one looks at this evidence, it is apparent that the Committee has gone somewhat further than Kahn did. Kahn stated his main thesis on this point in the following way:

> Within wide limits it is possible to achieve any desired structure of interest rates *by a suitable combination* of monetary policy with management of the National Debt. The Exchequer, by issuing short-dated securities in the place of long-dated, or *vice versa*, can secure the desired shift in relative rates of interest *against the background of a monetary policy which operates on rates in general.*[88]

Now Kahn further added that the authorities, in conducting a monetary policy, should not control the money supply *"as an end in itself"* but only because the money supply, other things given, determines the general level of interest rates.[89] He underlined this by saying:

[81] MinE, 9366.

[82] R, 374, 393.

[83] R, 395, 514, 603.

[84] R, 499, 395.

[85] R, 562.

[86] R, 395.

[87] R, 393.

[88] M-3, 145. My emphases.

[89] M-3, 145. His emphasis.

> It is the lower level of interest rates, not the larger quantity of money, which exercises an expansionist influence . . . it is immaterial what changes in the quantity of money have to occur as part of the process of securing a particular desired behaviour of rates of interest.[90]

Kahn is clearly advocating a monetary policy that controls the money supply for the purpose of setting the general level of rates, and a debt policy that controls the composition of debt for the purpose of setting the rate structure. But, presumably because of Kahn's oft-repeated assertion that the money supply is not important as an end in itself, the Committee seems to have been misled into believing that he was de-emphasizing the link between money and interest rates, and thus removing money from any central role in policy decisions, that he was putting all of his eggs into the debt-management basket, and that he was advocating the structure of rates as the "centre-piece" of monetary action. In any case, the Committee in its *Report* and in its questioning of witnesses seems obsessed with the idea of debt managers "doing something" about the structure of rates, while control of the money supply as a means of influencing the level of rates, or the price level, receives only passing mention, and then usually for the purpose of ridiculing the notion.

Presumably, then, debt management should operate in this way: the managers set the general level of interest rates by forcibly playing on the expectations and confidence of the public; they then go to work on the structure of rates by manipulating the composition of debt. In pursuit of this policy, debt managers may be called upon at times to change the level and structure of interest rates markedly. Also, in view of the record of the 1950's, rates would have to fluctuate very widely to affect spending directly. Are such fluctuations desirable? In answering this, the Committee states that if the wide fluctuations could be confined to the short-end of the rate structure the case for this policy would be fairly strong. But, since movements in short-term rates cannot much influence spending, either directly or indirectly, it is the long-term rates that must move. And there are three reasons why this policy should be rejected. First, while long-term rates can be raised quite high, the reverse policy of lowering them sharply during recessions would require a flood of liquidity into the economy that would play havoc with attempts at stability when the economic climate reversed itself;[91] and this liquidity would at the same time inspire a speculative swing against sterling. Second, widely fluctuating long-term rates would gravely weaken the foundations of financial institutions, by involving them, when rates are rising sharply, in capital losses on large blocks of securities.[92] Third, changes in long-term rates, no matter how marked, probably have little direct impact on spending in the short run.

[90] M-3, 144. See also Kahn's testimony, MinE, 10983-87.

[91] This statement recognizes the connection between liquidity (lending) and the level of interest rates, and it is therefore in opposition to what I have called the dominant view of the *Report* on this matter.

[92] R, 487-91. N. Kaldor, in his memorandum, advanced an additional argument against highly fluctuating rates: namely, that the average rate over time would then be higher, due to risk considerations, which would push the economy more toward consumption and away from investment, and so slow down its growth rate. He also felt that saving, under these conditions, would be allocated less efficiently to investment alternatives. M-3, 148.

Inasmuch as short-run movements in interest rates, in the Committee's opinion, can get at spending only through imposing capital losses on financial institutions' security holdings, and inasmuch as this policy is rejected partly because it would "gravely weaken" the foundations of these institutions, where do we go from here? Well, the Committee states, interest rates do have a direct effect on spending in the long run, and they do influence institutional lending in the long run, so they should be used carefully and slowly in a way to set the general long-term tone of the economy.[93]

> Our conclusions on rate of interest policy are therefore that, while there can be no reliance on this weapon as a major short-term stabilizer of demand, the authorities should think of rates of interest—and particularly long rates—as relevant to the domestic economic situation. The authorities should not aim at complete stability of interest rates, but should take a view as to what the long-term economic situation demands and be prepared by all the means in their power to influence markets in the required direction.[94]

To fill the policy gap in the short run, the Committee would like to see more use made of fiscal policy, as a short-run stabilizer (after some of its present defects are corrected), leaving monetary policy to act on the longer-term situation.[95] Thus, the standard policy prescription is turned on its head; it now reads: Use fiscal policy to iron out short-run fluctuations and monetary policy to guide the economy for the longer period.[96]

So much for poor old monetary policy in ordinary times. However, during an emergency, when the danger is that of "headlong inflation," monetary measures should be used vigorously. This does not mean restriction of the money supply—for that is not important—but rather striking "more directly and rapidly at the liquidity of spenders."[97] Now, since most of us are immediately inclined to identify the money supply as a major part of liquidity, these pronouncements seem contradictory, until we remember that, for the Committee, liquidity often refers only to lending (or borrowing). So it comes as no surprise that the monetary measures the Committee has in mind in such an emergency are direct controls of capital issues, bank advances, and consumer credit—and perhaps the control of lending by nonbank intermediaries.[98]

The role envisaged for monetary policy, in ordinary times, as modest as it may be, nevertheless requires that the authorities take positive action from time to time to influence the level and structure of interest rates. The Committee notes that during most of the 1950's the monetary authorities concentrated on short-term rates and were unwilling to admit "there was much

[93] R, 492-97.

[94] R, 498.

[95] R, 516-17. See also the Committee's discussion of this with the Treasury MinE, 13311-22.

[96] Along these lines, for what is probably the most extreme statement ever made on the relative merits of fiscal and monetary policies, see the memorandum by I. M. D. Little, R. R. Neild, and C. R. Ross, M-3, 159-67.

[97] R, 524.

[98] R, 520-29.

scope for the exercise of official influence over long-rates"; it recommends that the authorities give a more positive lead in the long end of the market.[99]

This seemingly innocent recommendation was actually the upshot of a running debate between representatives of the Bank of England and the Committee that was one of the hottest of the hearings, rivaling even those of recent years in this country between the Federal Reserve and its Nemesis in Congress. The Bank authorities defended, with the greatest of moral fervor, the doctrine that they do not, and most certainly should not, influence in any direct way the level of long-term rates; and, moreover, at times they argued that they should not exert such influence, except on a very temporary basis, in any indirect way, such as by operating at the short-end of the market. They simply "follow the market," permitting the natural forces of supply and demand to determine the level of rates. Spokesmen of the Bank were never ready to concede that operations at the short-end were meant to influence the entire rate structure, though they did think that at times their operations probably did have some impact on long-term rates, undesirable and annoying as that may have been.[100]

At one stage, Bank officials were asked why, in view of strong inflationary pressures, they did not act more energetically in getting long-term rates higher. The answer and subsequent exchange follow:

> I want it to be quite clear that we do not set out to intervene in a trend.
>
> Even though it was reasonably plain that the reason for the action was to establish a long-term rate of interest that was needed to the general economic health of the country? —— Yes, even doing one's best to make it plain that "it hurts me more than it hurts you."[101]

Bank officials felt that their operations had not in fact pushed up long rates, that these rates had increased because of the public's fear of inflation,[102] and that it was really the impersonal forces of supply and demand that had "a pretty big influence" on long-term rates.[103] Under questioning, however, it was admitted that the Bank could affect both supply and demand, but the officials made it clear that under no conditions would they wish to do this in any direct way, because such a policy would lead the public to cry "stinking fish" at government securities and would damage Her Majesty's Government's credit. But the officials were hard put to defend this position:

> When you say that "it would greatly damage the Government's credit," what effect have you in mind there? —— Put briefly, that if we have just issued a new stock at, let us say, 100, and we then proceed actively to sell it down ourselves to 95, we have largely by our own direct actions on that security forced a book loss of five points on the people who took the security at 100.

[99] R, 428.

[100] This is noted in R, 428, 552, 583. The Committee states that "we were sometimes assured that the bill rate has practically no connection with long rates."

[101] MinE, 1884, 1887. See also MinE, 1792, 1796, 1804, 1845.

[102] MinE, 1849-51.

[103] MinE, 1870.

It operates in a similar way in your sales of Treasury Bills from day to day; the Bank Rate will very definitely affect the Treasury Bill rate. You impose a loss on all holders of Treasury Bills? —— I accept that, but that is the recognized play of the money market.

Why is the one transaction objectionable and the other not? —— The Treasury Bill and short bond market is a technical market, where these things are clearly understood . . . [The institutional market is different]; if, shortly after you bought [a security] at 100, you found someone else buying it at 92 or 93 your confidence would be very seriously reduced. We cannot afford to impair people's confidence. . . .[104]

VII. *The Bank's Status, Sterling, and Statistics*

In the foregoing, I have neglected several areas that received much attention in the *Minutes of Evidence* and that were dealt with at some length in the *Report*. There is space only to skip across the high points of three of these areas.

A. *The Status of the Bank of England*

The Committee rejects the theory that the Bank of England should be completely independent of political influence.[105] It believes that the function of the Bank "is to act as a highly skilled executant in the monetary field of the current economic policy of the central Government," and that "the policies to be pursued by the central bank must be from first to last in harmony with those avowed and defended by Ministers of the Crown responsible to Parliament."[106] The Bank should generate advice, views, and proposals of its own, but the will of the government should be paramount.[107]

The Committee's opinions on the status of a central bank were not shared by Riefler, whose comments on this subject prompted the committee to ask:

This means, leaving Congress on one side, that, as between the Administration and the Federal Reserve on an issue of policy, the initiative and the last word rest with the Federal Reserve? Suppose that the Administration want a certain economic climate to be created, they can merely discuss their desires with the Federal Reserve? —— The only question that comes up relates to the reading of the business and credit situation. Sometimes there are differences of view. The question then is whose judgment is going to prevail. Our position is that obviously the people who are more specialized in reading the business and credit situation have to make the judgment. It would make no sense to have us try to make a judgment on what the business and credit demand is, and then

[104] MinE, 1849, 1855-60. For a fuller discussion of these questions, see MinE, 1762-63, 1792-1805, 1841-98. The Committee's views are in R, 551, 575-76. The Treasury's position was much the same as the Bank's; see MinE, 2387-98, 2953-72, 2799-2995.

Subsequently, the Bank did use open-market operations to influence long-term rates. For a justification of this and a slight modification of its initial position, see MinE, 11919, 12000-01, 12008-14, 13416, 13453. This change is discussed by the Committee in R, 341, 428, 553.

[105] R, 768.

[106] R, 767, 769.

[107] R, 761-62. See also MinE, 256-58.

have somebody else super-imposing another judgment. It ought to be made by whoever is most capable of making it.[108]

Riefler returned to the point later:

> If the System is in this way by statute independent of the executive, and therefore from direct political pressure, and also independent of private interest, is the argument ever heard that, by being so insulated, they are in an ivory tower and out of touch with what is going on in the length and breadth of the United States? —— Yes, Mr. Elliott Bell does say that in *Businessweek* [sic]; anyone wishing to criticize the system is very likely to say that. I do not think it is taken very seriously. The system, through the directors of the Banks and the branches, and through its contacts, is intimately bound into the structure of the economy. In a sense it is the most important recorder of the state of the economy of the country; often the same people who are opposed to some action taken and who raise the cry of the "ivory tower" rush to us to corroborate any judgment they have on economic questions.[109]

The *Report* recommends the formation of a Standing Committee which would be advisory in character; it proposes that changes in the Bank Rate be made in the name of the Chancellor; and that part-time directors of the Bank be retained, a subject that had been discussed at length before the Parker Tribunal.[110]

B. *International Aspects*

The Committee says nothing in this portion of the *Report* that will upset foreign confidence, steps gingerly around many touchy issues, and generally approves of official postwar policy toward sterling.

It has little confidence in the ability of changes in short-term rates to cor-

[108] MinE, 9407.

[109] MinE, 9454. Compare Riefler's statement with that of the Governor of the Bank of England, who said in part: "I have no doubt that in modern conditions it is proper that Government should have the final word on policy and that the central bank should not be free to pursue a completely independent line." MinE, 12813.

[110] R, 771, 773, 778-87; MinE, 262-63, 269. A similar body has been suggested here many times in recent years, but the proposal has been attacked partly because it would jeopardize the independence of the Federal Reserve. Four years ago, before the Joint Economic Committee, Elliott Bell submitted a remedy:

> If . . . it is felt that the Federal Reserve Board is so sensitive that contact with the President would corrupt it, then I suggest there might usefully be formed a National Economic Council without regular representation by the Federal Reserve Board. In this event, the Fed might be invited to send an observer with the express understanding that he could sit near an open door ready to fly to the sanctuary of Constitution Avenue if he felt the danger at any point of political contamination.

See *Hearings before the Subcommittee on Economic Stabilization of the Joint Economic Committee*, on December 10-11, 1956, p. 7.

The Committee received and heard an unusually large amount of evidence concerning the status of the Bank. For the Bank's views, see M-1, 5-9; MinE, 249-86, 752-60, 12813-900. For opinions of part-time directors of the Bank, see M-1, 44-45; MinE, 12066-188. Riefler's statements are found in MinE, 9395-9407, 9422-36, 9452-54. The views of former Chancellors and others are found in M-3, 47-48, 70-71, 207-11, 248-49; MinE, 11250-99, 12301-640.

rect balance-of-payments difficulties. "We have had little evidence of actual movements of funds in response to changes in short-term rates . . . ," though it adds that there is some indication that a rise in long-term rates has induced purchases of long-term securities in London. However, "the fact that changes in rates have had only a limited effect on the movements of funds in a period when sterling was weak does not imply that they will be correspondingly ineffective if sterling is strong." Rather than relying on movements of interest rates, it perhaps would be better at times to support the forward rate of exchange, though many weaknesses of this proposal are noted.[111] The Committee favors a fixed parity for sterling, rejecting freely fluctuating exchange rates, which were advocated by James E. Meade, and devaluation as a policy, "though as a way of escape it cannot be excluded."[112]

Despite the recent improvement in reserves, the Committee states that they are still far from adequate; but it is noted that the U.K.'s problem in this respect is really part of a world-wide "shortage" of international reserves relative to levels of trade.[113] This raises the question of whether there should be a substantial increase in the world price of gold, a proposal that is rejected by the Committee as not "immediately necessary [nor] the most helpful approach to the problem of international liquidity." Moreover, an increase in the price of gold would alter the existing distribution of reserves "very much in favor of the countries that are most amply provided."[114]

It is felt that this problem can best be attacked by utilizing existing international machinery, such as the International Monetary Fund, but the Committee believes that in its present form the IMF has many defects. Several suggestions are offered to remedy these defects, including relaxing the requirements on drawing rights, but the Committee seems more enthusiastic in suggesting that the IMF might be turned into an international central bank, with its own unit of account, along the lines originally suggested by Keynes.[115]

The Committee approves the steps taken toward convertibility and nondiscrimination in trade. It is hopeful about the international position of the United Kingdom, and believes that the dollar problem "is likely to be more intermittent and less intractable than is sometimes supposed, and that it has already changed in character, and is likely to continue to do so."[116] It is reported that the Treasury is looking ahead to a larger current account surplus in the early 1960's.[117] This is all to the good, because it is necessary "to maintain a balance of payments sufficiently favorable to leave a margin for loans and other investments. This margin must be correspondingly widened to allow of grants in aid of colonial development." Since, however, it is better for the United Kingdom to use its surplus more to build up reserves than long-term

[111] R, 695-702, 703-07. See the Treasury's spirited objection to the support of the forward rate in M-1, 121-22.

[112] R, 716, 719-22, 728.

[113] R, 670-71; MinE, 2531.

[114] R, 672-74.

[115] R, 678.

[116] R, 684.

[117] R, 630, 734.

assets, other sterling countries should be encouraged to borrow more in other areas of the world.[118] But the United Kingdom should remain a major source of capital to the Commonwealth even if it has to borrow abroad for this purpose.[119]

C. *Financial Statistics*

Although many trade associations, the Bank of England, the Treasury, and others submitted economic data to the Committee that had not previously been published,[120] the Committee was frequently handicapped by a lack of information on matters into which it was inquiring. The Committee discussed this problem in its *Report* at some length and carefully pointed out the means of correcting it.[121] As anyone who is at all familiar with the paucity of British economic data might guess, the *Report* called for more reporting and publishing of statistical series in almost every field of conceivable interest to the authorities and the informed public. This required a certain amount of courage on the Committee's part, considering what it went through in its examination of some witnesses. To give the reader some idea of this, I cannot do better than follow through the trials and tribulations of the Committee in its attempt to gain one small piece of information—that on the distribution of investments held by commercial banks. The Committee, early in the game, confronted the Bank of England:

> What information does the Bank of England now receive from the clearing banks about their investments? —— I should like to take that one away, Mr. Chairman; you see, the banks are our customers.[122]

Rebuffed there, the Committee turned next to the Treasury, in this way:

> Then is it the case that the holdings of the clearing banks, in the categories, say, nought to five year bonds, five to ten, ten to fifteen, or whatever the broad categories may be, are not known in aggregate either to the Bank of England or to the Treasury? —— There is no regular return to my knowledge; but it is not true that we do not know.[123]

Since the Treasury knew but was not telling, the Committee raised the question with the London clearing bankers:

> It is suggested that the aggregate figure of the distribution of the clearing banks' investments may be an important thing from the point of view

[118] MinE, 2492-2507, and p. 956.

[119] R, 741-47. The Bank's views on the international aspects in M-1, 13-17, 34-35; MinE, 833-947, 948-71; the Treasury's in M-1, 105 112-22; MinE, 2483-2615, 3211-22, 9695-9734. Economists addressed themselves to these issues in M-3, 71-76, 132-36, 243.

[120] The Bank presented a memorandum on the current sources of banking statistics (M-1, 66-70); the Central Statistical Office prepared data on sources of financial and economic statistics relating to the monetary system and on assets of financial institutions (M-1, 129-62); The National Institute of Economic and Social Research presented a comprehensive memorandum on financial and economic statistics (M-3, 3-27); and the many data presented by trade associations are found throughout the evidence.

[121] The recommendations are found principally in Ch. 10 of the *Report;* but see also R, 366-67, 580, 582, 629.

[122] MinE, 167.

[123] MinE, 2841.

of an informed study by the public. The question is: could that have any adverse effect upon the interests of the banks, as banks? —— [Mr. Robarts] I should like to consult Mr. Tuke on this point. [Mr. Tuke] I have been brought up in an atmosphere of not disclosing our affairs or, so far as possible, any of these things; and (though perhaps I have no right to say this) I should want to be convinced that it really was in the interests of the country that these things should be disclosed. If I was told that there was, a sufficient reason, other than what I would call curiosity to know about these things, then it might outweigh our disinclination to disclose figures. But we have this natural disinclination.

A great deal of our discussion this morning had to do with the liquidity of the banks. Would you not agree that in that connection it was of interest to know something about this? —— Mr. Robarts has told you the sort of way in which we try to divide these investments up in pretty well equal tenths. I would support that. That is our policy, always, in practice, with a bias at the lower end; there is more under five years than over.

Would there be any more significance in knowing the proportion of your investments under five years than in knowing the money at call and short notice, which you do disclose? —— If we give you the figure of stocks under five years, someone will come along and ask for more information; they will want to know the figure for stocks under two years.[124]

Undaunted, the Committee later tried out the Scottish bankers on this delicate issue:

Do you not think that it would be useful if figures for the distribution of your investments by maturities were available to the public? —— This question rather takes me by surprise. If . . . it appeared in a certain month that the Scottish banks were going longer in investments, what reaction would the public have to that announcement?

Let us for the moment leave the public out. Suppose that you were a monetary authority and concerned to operate a monetary policy on behalf of the Government; would not the availability of the figures on this question be important to you? —— It might quite well be important, and the Scottish banks would probably be willing to give this information, if it were known that it was not for public dissemination.[125]

Finally, about a year later, the Committee raised the question again with the London clearing bankers, with the following disastrous results:

Subheading (ii) also relates to the division of your figures of holdings of gilt-edged securities between under five year bonds and over five year bonds. Would you see objection to disclosure of that to the public at some interval? —— I would see the very greatest objection.

You observed this morning that I was in some doubt whether any substantial change had taken place since before the war in this distribution. I imagine that, if we are in doubt, the general public would be still more in doubt? —— The general public would not think anything about it.

[124] MinE, 3862, 3864-65.

[125] MinE, 5042-43.

I was referring to the informed public. There have been discussions of this in the financial Press? —— Not altogether well informed, if I may say so.

Would it not be better if they were well informed? —— We are not at all keen on the idea of publishing them to all and sundry.

What is the reason for this? —— Everything can be picked up by certain financial journalists who will fasten on anything which could in any way be used to the detriment of the banks' wellbeing.

You are giving more rope to the people who make uninformed comments. Is that what you really want? —— *We do not want the public to discuss our affairs.*[126]

That should be enough to indicate the nature of the barriers that faced the Committee, but I cannot refrain from reporting one more exchange. The Committee suggested to the clearing bankers that it would be of interest to have quarterly figures analyzing bank deposits as to holder. The bankers' answer to this was:

Professor Cairncross used the word "interest"; if I may say so without disrespect, if it is merely a question of interesting academic economists I am not very anxious to spend a great deal of money on that.

Do you not think that academic economists have contributed to an understanding of monetary policy? —— They all quarrel amongst themselves, so that one does not know what to believe.[127]

Finally, for anyone interested in further pursuit of this matter of statistics, he could do no better than follow the Committee in its quest for the marketable security holdings of the Exchange Equalization Account in 1939, a real thriller with a surprise ending.[128]

VIII. *Concluding Remarks*

An English visitor here, when asked what he thought of the Radcliffe *Report*, replied that it was "woolly." He undoubtedly meant that it was confused and hazy. But there is a colloquial meaning of the word, which is "attended with unusual excitement," as a woolly melodrama. The *Report* is certainly woolly in both senses.

It is exciting because the Committee's undertaking was hazardous from the very beginning, being nothing less than the development of a general theory of finance that would explain the impact of financial variables on the postwar British economy. And it must be said, considering the string of celebrated witnesses that paraded before it, that the Committee received surprisingly little help in its main task. In view of all this, not to mention time limitations, the *Report* it turned out, for all its deficiencies, is remarkable. The macrocosmic view the *Report* gives of the world of finance will leave many monetary theorists and policymakers uneasy in their self-imposed exile to a small corner

[126] MinE, 13247-52. My emphasis.

[127] MinE, 13234-35.

[128] MinE, 2848-55, 3223.

of this world. If it does nothing more than open up this larger world to them, it will have served a worthy purpose.

But at the same time, in its analysis of this wide, wide world there seems to be confusion everywhere—in the role of the money supply, in the concept of liquidity, in the analysis of nonbank intermediaries, in the discussion of interest rate determination, in the exalted role assigned to debt managers, and so on. Even so, though this is the *Report* at its worst, it can still be judged an honorable failure. "Honorable" because, as Zarathustra reminded the dying rope-dancer, there is nothing contemptible in making danger one's calling, nor in perishing in that calling. Nothing contemptible; but still, in the way it all ended, something distinctly sad.

COMMUNICATIONS

The Public Debt: A Burden on Future Generations?

"Personally, I do not feel that any amount can be properly called a surplus as long as the nation is in debt. I prefer to think of such an item as a reduction on our children's inherited mortgage."

—President Eisenhower, *State of the Union Message*, January 7, 1960.

Two things are certain. The first is that, whatever else this quotation from President Eisenhower's State of the Union Message may imply, the President appears convinced that the costs of debt-financed public projects can be passed on to future generations. The second is that the popular economics textbooks of our day are nearly unanimous in their rejection of this "naive" view of the public debt. The purpose of this brief note is to suggest that in this instance it is the President who is—in at least one highly important sense—right.[1]

The basic question at issue seems simple indeed: Can the "real burden" of a public project financed by a privately held internal debt be shifted from one generation to another? The usual economics textbook answer to this question is that the burden can *not* be shifted to future generations because government spending must drain real resources from the community at the time the government project is undertaken (assuming full employment of resources) regardless of whether the project is financed by borrowing, taxes, or money creation. As Samuelson puts it: "To fight a war now, we must hurl present-day munitions at the enemy; not dollar bills, and not future goods and services."[2]

What is wrong with this by-now-standard argument? Absolutely nothing, if the real burden of the debt is defined as the total amount of private consump-

[1] J. M. Buchanan [1] is one of the few contemporary economists to argue in favor of the proposition that the real burden of a public debt *can* be shifted to future generations. It was Buchanan's stimulating book that started the train of thought that has resulted in the argument contained in this paper. The reason for the present paper is that while Buchanan has arrived at essentially the same conclusion, he has apparently not succeeded in convincing very many people that he is right—at any rate, he has not convinced several reviewers of his book (see, for example, the reviews by Rolph [5], Lerner [4], and Hansen [2]). Perhaps the reason these reviewers have not accepted Buchanan's conclusion on this point is that Buchanan: (1) does not always define "real burden" in a sufficiently clear manner; (2) defines "generation" in such a manner that the same person can be considered a member of many different generations [1, pp. 33-34]; and (3) relies on what Rolph [5, p. 184] has called a "proof by indirection." We have tried to avoid these pitfalls.

[2] [6, p. 351]. Among the widely used elementary texts, C. L. Harriss' book [3, pp. 689-97] seems to come the closest to accepting the line of argument presented here. However, Harriss' exposition is badly impaired by an unclear distinction between "real costs" and "money costs." All writers seem to agree that the so-called "transfer payments" necessitated by a public debt involve real burdens in the sense that taxes used to meet interest payments may impair incentives to work and save. Neither this aspect of the debt problem nor the relationship between the public debt and economic stabilization are discussed in this paper.

tion goods given up by the community *at the moment of time the borrowed funds are spent.* Under this definition of real burden, the cost of the public project simply must be borne by the generations alive at the time the borrowing occurs.

There is, however, another definition of real burden which permits, under certain circumstances, present generations to shift the burden to future generations. And this definition, we submit, is a more accurate representation of the everyday notion of burden and is a more sensible concept for deciding if the real cost of a certain project can or can not be postponed to future generations. Let us define the real burden of a public debt to a generation as the total consumption of private goods foregone *during the lifetime* of that generation as a consequence of government borrowing and attendant public spending. (For the moment, we are not taking into account the benefit that may result from the public expenditure, and so we are talking about a "gross burden.") Our preference for the lifetime of a generation as the unit of account is based on the proposition that people can and do forego consumption at a moment of time in order to be able to consume more later, and that to use the amount of consumption foregone at any one moment of time as some sort of index of the over-all sacrifices made by a generation is misleading.

Let us now consider the following situation. Assume a full-employment economy. Assume further that there is within the society an identifiable "generation" of people, all of whom are, let us say, 21 years old. Suppose that at a given moment of time the government sells bonds to the private sector of the economy in order to finance public project X[3] and that all of these bonds are voluntarily purchased by the group of 21-year-olds, whom we shall refer to as Generation I.

To determine the allocation of the burden of public project X between generations, consider a point of time 44 years later when all members of Generation I are 65 years old and the rest of the community is made up of a Generation II, whose members are all 21 years old. Suppose that at this moment of time all the members of Generation I who own the still outstanding government bonds sell these securities to members of Generation II and use the proceeds for the purchase of consumer goods during retirement.

In this case it is clear that the lifetime consumption of the members of Generation I has not been reduced even though the total subtraction from the production of private goods due to the carrying out of public project X took place during their lifetime. The reason is simply that the saving represented by Generation I's original purchase of the bonds has been matched by the dissaving resulting from the later sale of the bonds to Generation II and the subsequent spending of the proceeds. Conclusion: Generation I has not assumed any of the burden entailed in financing public project X by the issuance of government bonds. (For the time being, we ignore the interest charges on the debt.)

Let us now examine the situation of Generation II. If the government makes

[3] At this juncture, the precise characteristics of the government project are best left unspecified. The relevance of the particular type of government project undertaken will be considered shortly.

no effort to retire the debt during the lifetime of Generation II, and if Generation II sells its bonds to Generation III, then Generation II also escapes the burden of paying for the public project, and so on. To make a potentially long story short, suppose, however, that during the lifetime of Generation II the government decides to retire the debt by levying a general tax in excess of current government spending and using the surplus to buy up the bonds that are now held by members of Generation II. The inevitable outcome of this decision is a reduction in the lifetime consumption of Generation II. The taxpayers of Generation II forego consumption in order to retire the debt and yet the bondholders of Generation II do not experience any net lifetime increase in their claims on consumption goods since they are simply reimbursed for the consumption foregone at the time when they (Generation II) bought the bonds from Generation I. Conclusion: the burden of public project X rests squarely on Generation II, and not on Generation I.

The skeleton of our argument is now complete: While the resources consumed by a debt-financed public project must entail a contemporaneous reduction in private consumption, the issuance of government bonds permits the generations alive at the time the public project is undertaken to be compensated in the future for their initial sacrifice. Generation I merely makes a loan of its reduced consumption, and the real reduction of consumption is borne by the generation(s) alive at the time this loan is extinguished. Consequently, even though the real private consumption of the community as a whole need not be altered by the growth of the public debt, it is still possible for the distribution of the community's private consumption *between generations* to depend on whether or not public projects are debt-financed.

One other form in which the general argument that no burden can be passed on to future generations often appears is the "we owe it to ourselves" or "assets equal liabilities" version:[4] No burden can be passed on because corresponding to every asset in the form of a government bond outstanding there is an equal liability in the form of liability for taxes to meet the interest charges and to repay the principal of the debt. Since there are always these two offsetting sides to the debt instrument, the argument proceeds, future generations can not be handed any burden since when our taxpayer-children inherit the tax liability our bond-holder children will acquire an equal asset.

The difficulty with this argument is simply that the asset and liability sides of the public debt are "passed on" in significantly different ways. In so far as the government bonds are acquired by Generation II by purchase rather than bequest, the recipients of the bonds have only received a *quid pro quo*. On the other hand, the members of Generation II who are handed the tax liability are not reimbursed for accepting this liability. From the vantage point of Generation I, the bondholders in this generation received claims on consumption in exchange for the asset (bonds) which they sold to Generation II; at the same time, the members of Generation I as liability-holders passed on their tax liability to Generation II by the simple expedient of dying, and thus did not have to give up consumption goods to get rid of the liability.

[4] For a fuller exposition of this line of argument, with references to the literature, see [1, pp. 4-14, and *passim*].

Consequently, unless Generation II can in turn pass its assets on to Generation III by sale while at the same time passing on its liability without making a compensating payment, the burden of the debt will be borne by Generation II.

We come now to the question: How about interest payments? We shall argue that the interest payments on the debt represent some burden on each and every generation that must pay taxes to make such payments.

To show why this is so it is necessary to reconsider the meaning of our definition of real burden—namely, "the total consumption of private goods foregone during the lifetime of a generation." Thus far we have implicitly assigned all amounts of consumption enjoyed during the generation's lifetime equal weights in arriving at total lifetime consumption, and have disregarded entirely the stage in a generation's lifetime at which various amounts of consumption were enjoyed. Consequently, we were able to argue that a generation which gave up a certain amount of consumption early in life to buy bonds and then was able, by selling the bonds later in life, to enjoy the same amount of consumption during retirement years had avoided all of the burden involved in the debt financing of project X. The difficulty with this treatment is obvious. So long as people have a positive rate of time preference (that is, prefer present consumption to future consumption), they will feel that they have made a sacrifice if they give up a certain amount of consumption in their youth and then receive back exactly the same amount of consumption in their old age.

If we assume that the interest rate on the government bonds approximates the generation's rate of time preference, then the interest payments on the national debt serve to compensate the owners of the debt for their willingness to forego consumption early in life, and thus (along with the recapture of the principal late in life) serve to make the discounted value of the lifetime consumption of the bondholders the same as it would have been if project X had never been contemplated.

Turning now to the tax side of the interest transaction, it is clear that the tax payments needed to make interest payments represent a real reduction in the lifetime consumption of the people paying the taxes. Furthermore, since any given year's debt service is paid out of approximately contemporaneous tax payments, the same generation that receives the interest payments will be making a large part of the tax payments. The inescapable conclusion is that while the interest payments (along with the repayment of the principal) do not increase the discounted lifetime consumption of a generation, the tax payments do decrease lifetime consumption. Consequently, the discounted lifetime consumption of the generation is, on balance, reduced by the existence of debt service. This burden represents the real loss of welfare incurred by the generation as a consequence of the fact that it postponed its consumption but did not—because it received in interest payments only what it paid in taxes—receive any compensation for this distortion of its preferred consumption pattern.

The reason that in our discussion of interest payments we have spoken of "a" generation or "the" generation is that this burden (measured now in

terms of the reduction in the discounted value of the generation's lifetime consumption) is borne, of course, by each generation that pays a service charge on the debt. Consequently, even if the principal value of the debt is continually passed on, each generation bears a burden in the form of an uncompensated distortion of its preferred pattern of consumption.

So far we have avoided consideration of the type of government project financed by the initial borrowing. Actually, whether the government funds are spent wisely or foolishly is largely irrelevant to the question at issue here; for we are concerned solely with the allocation of the real *cost* of debt-financed government spending between generations and not with the allocation of the benefits of the government spending over time. Consequently, our conclusion that it is possible to shift at least a part of the cost to future generations does not imply that the absolute well-being of the future generations has been worsened by the combined borrowing and spending operation. If the borrowed funds were spent on a project whose benefit stream extends far into the future (for example, on fighting a "war-to-end-wars"), then the generation that assumes the main burden of the debt may still be much better off than if the debt had never been incurred. Our point is simply that the use of borrowing—as opposed to taxation or money creation—has improved the lot (measured in terms of lifetime consumption) of the first generation relative to the lot of succeeding generations.[5]

There is one final qualification to our argument. We have constructed a somewhat simplified case by assuming that all the bonds held by Generation I are sold to Generation II and that the proceeds are used entirely to increase consumption during the remaining years of Generation I's life. If, for example, all the members of Generation I were to will their bonds to Generation II, all real sacrifice of consumption would be borne by Generation I. Nevertheless, in spite of this simplification, the argument undoubtedly contains a large measure of relevance for the real situation. Purchasers of bonds, during a war for example, lose current consumption and receive marketable securities which surely are at least in part intended for conversion into spending on consumables in later years. The resulting claims upon consumer goods are realized at a time when they draw against the productivity of new members of the community who would otherwise enjoy a higher level of consumption. The existence of the marketable bonds undoubtedly makes possible at least some transfer of real income between generations.

Our conclusion that the real cost of debt-financed government spending can (at least in part) be transferred to future generations does not, of course, establish any prima facie case against deficit financing or in favor of the

[5] There is a second, closely related reason for not tying the argument of this paper to a particular government expenditure, whether it be the construction of public schools or the giving of an enormous fireworks display. If, at the time public debt is issued, the government is spending money for many activities and financing these activities by taxes (and perhaps by money creation) as well as by borrowing, then it is hard to see how one can impute any specific project to any specific method of finance. The fact that we cannot solve this version of the imputation problem is irrelevant to the basic proposition that the cost of debt-financed government projects can be passed on to future generations, and thus cannot be used to disprove this proposition.

prompt retirement of the national debt. For one thing, to the extent that public projects undertaken today aid future generations, it may be fairer to let these future generations help pay the cost of these projects than to put the entire burden on present generations. Furthermore it is obvious that many considerations other than the location of the debt burden—such as the employment situation, the needs of the country for collective consumption, and the effect of taxes on incentives—are relevant in determining budget policy. However, at the present moment, there seems to be less danger that economists will forget the importance of these other considerations than that they will deny the possibility that the public debt can be used to shift a part of the real cost of public projects on to later generations.

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Capital Formation in Underdeveloped Countries

In the rapidly growing volume of literature on the problems and prospects for economic development, considerable attention is being devoted to the determinants of net capital formation or, more particularly, to the obstacles to and limitations upon capital formation in underdeveloped countries. This preoccupation is, without question, well deserved. One need not subscribe to a monocausal theory of development to argue that an increase in the percentage of annual output devoted to investment is an urgent and indispensable prerequisite to a long-term rise in real per capita incomes. Indeed, current attempts at what is now being called, rather pretentiously, "development programming," often consist exclusively of measures designed to raise the over-all rate of capital formation and to exert some centralized guidance over the allocation of investment resources.[1]

A central question in a theory of development, then, is: Why are rates of

[1] "A programme of economic development is the expression of a simple idea, namely, the desirability of increasing and judiciously regulating capital investment, so that a stronger impetus and greater order may be given to the growth of the country" [11, p. 3].

capital formation as low as they apparently are in most underdeveloped countries? Economists can hardly be accused of having neglected this question. Indeed, it will be suggested that we now have too many explanations.

Explanations of capital deficiency are commonly organized in terms of two separate sets of forces: factors accounting for low saving propensities, on the one hand, and those responsible for weakness in the inducement to invest, on the other. However, such an approach leads to a preoccupation with certain phenomena and preference patterns which are commonly treated as independent causal agents, although there appear to be compelling reasons for regarding them as the superficial consequences of more fundamental factors.

It is proposed to show that certain preference and behavior patterns which are commonly observed in underdeveloped countries are in fact part of an elaborate adaptive mechanism geared to an economic environment characterized by a generally low marginal efficiency of capital and by a limited spectrum of profit opportunities. It will in fact be suggested that an adequate explanation of the general weakness of investment incentives and of the peculiar distribution of profit prospects in different sectors of underdeveloped economies would, *by itself*, account for many of the other apparent causes of low rates of capital formation. It will be demonstrated that the failure to "shake out the implications" of weak investment incentives has led not only to a double-counting of "causes" of low rates of investment but also to serious analytical errors and highly questionable judgments.

I. *Low Aggregate Savings*

The most obvious explanation for low rates of capital formation is the present poverty of underdeveloped countries. Their capacity to undertake productive investment appears to be sharply limited by the very low levels of per capita income combined with the frequently extravagant expenditure patterns of the rich and an interest, among virtually all income classes, in the acquisition of certain kinds of durable consumer goods.

What is not so apparent, however, is that individuals in underdeveloped countries may regard the acquisition of certain types of consumer goods as a form of personal saving and investment. This may well be the case for a variety of so-called luxury goods, especially such items as jewelry and precious ornaments which may constitute excellent stores of value in an environment characterized by political insecurity or inflationary pressures. Even where such conditions do not prevail, the absence of well-developed and reputable financial institutions may provide strong inducements for the acquisition of jewelry, gold or even foreign assets as relatively secure forms of personal saving. To the extent that this is so, our present measures of capital formation may significantly understate the capacity for saving and productive investment in underdeveloped economies. What appear to be low rates of saving in underdeveloped countries may be attributable, in part, to the fact that durable consumer goods are acquired and held not so much as the result of a real taste preference as because these goods are regarded as a form of personal saving in a backward economic environment [5, pp. 175-76].

A closely related and much more serious difficulty of a theoretical nature

arises over the common practice of explaining low rates of productive investment in terms of the failure of underdeveloped economies to generate a sufficient flow of domestic savings. The low rate of saving is in turn attributed to practices and preferences which squander the "potential economic surplus"—purchases of jewelry and gold, acquisition of foreign currencies and assets, lavish consumption expenditure patterns of the upper income classes, etc. In a certain sense such actions do help to explain the low rates of saving and productive investment, but only in the same sense as the statement "I prefer to stay home and watch television tonight" explains why I do *not* attend the local high school concert. It seems necessary to insist that statements of preference call for an examination and evaluation of the nature and comparative attractiveness of available alternatives. Precisely what is involved here is the unattractiveness of the alternatives, i.e., the unprofitable nature and/or high risk factor attaching to productive investment in underdeveloped countries. Low rates of saving and the absence of attractive investment opportunities are not independent of one another and cannot be treated as entirely separate and unrelated causes of capital deficiency. Rather, the actions referred to above may, to a large extent, be attributed to the "undeveloped" nature of the investment mechanism and to the complex of forces which weaken the inducement to invest [3, Ch. 2].

The problem discussed here cannot be dismissed as of a "Which came first—the chicken or egg?" nature—not, at least, without discarding much of Keynesian economics. That is to say, the same forces which account for the weak inducement to undertake productive investment may also account, in large measure, for the unproductive disposition of potential savings; and policies which have the effect of raising the marginal efficiency of capital schedule and therefore increasing the inducement to invest may reasonably be expected to induce, at the same time, an increased flow of (*ex ante* as well as *ex post*) domestic saving.

Implicit in much of the current discussion, however, is the classical assumption that the real limitations upon investment lie in the limited willingness or capacity of the public to save and that, therefore, an increased propensity to save is all that is needed to generate an increased flow of productive investment activity. Such analysis, however, often not only ignores the current weakness in the inducement to invest, but may also overlook the fact that an increased propensity to save, by its adverse effect upon the marginal efficiency of capital schedule, may weaken the inducement to invest even further [4, pp. 210-13].

Moreover, the priority which is usually attached to the need to increase savings as a precondition for raising the rate of capital formation really involves the concealed premise that an increased rate of capital formation ought not to be achieved at the expense of domestic inflation. Obviously, all measures or policies which result in a transfer of resources from the production of consumer goods to the production of investment goods necessarily generate an equivalent amount of *ex post* savings. This is not to ignore the many cogent reasons for avoiding the inflationary route, but merely to sug-

gest that the currently fashionable way of stating the problem may be seriously misleading.

II. *Methods of Holding Assets*

The preoccupation with the low level of domestic saving in underdeveloped areas as an independent determinant of low rates of capital formation creates a host of difficulties in attempting to move from the micro- to the macroeconomic level of analysis. The general problem involved here is the macroeconomic consequences of certain kinds of asset preferences on the part of individual savers. It is important that we distinguish between (a) factors which affect the aggregate volume of savings, and (b) the *forms* in which an individual chooses to hold his assets.

It is frequently held, e.g., that when an individual chooses to hold a net increase in his assets in the form of liquid balances (i.e., "hoarding") that this does not result in genuine saving in an aggregative sense, i.e., in the release of resources for nonconsumption purposes.

This argument appears to result partly from the failure to distinguish between the hoarding of cash balances and the "hoarding" of physical assets —jewelry, gold, or other durable commodities—the acquisition of which may involve a drain upon scarce domestic resources (or the supplies of foreign currencies, if imported).[2]

The hoarding of cash balances, even in the absence of appropriate financial institutions, will nevertheless result in the release of resources which might otherwise have been used for consumption purposes and therefore creates an added potential for capital formation.[3] Obviously, such hoarding does not ensure that resources will be devoted to investment purposes. The deflationary consequences and the unfavorable impact upon interest rates and the inducement to invest may simply result in unemployment and an increase in the volume of unused resources. The point is that hoarding of cash balances in no way absorbs real resources but releases them from consumption uses, just as do other forms of saving. To the extent that savers decide to add to their stocks of liquid assets, the possibilities for raising the rate of capital formation are increased, and may be realized, for example, in a noninflationary fashion by deficit spending on the part of the government. Whether such possibilities *are* realized is not at issue. The essential point is that hoarding of cash balances, in itself, releases rather than absorbs real resources.

Although the hoarding of cash balances does not reduce the volume of sav-

[2] "Though the potential of capital formation in underdeveloped regions seems considerable when judged by the high share of output retained by the landowner, actual capital formation goes on at low rates. Oriental landlords have been known for generations for their high propensity towards 'unproductive' use of accumulated revenues. Even if the money is not spent in travelling abroad, it is invested largely in hoards of cash, jewellery and gold" [2, p. 38].

[3] Of course it is possible that the absence of secure financial institutions discourages the saving habit and that individuals would save more readily if they had convenient access to a bank or other saving institution. Moreover, the absence of adequate financial institutions may also partly account for the habit of acquiring and holding goods as a liquid asset.

ings it may have an important consequence in limiting the availability of financial resources to certain potential borrowers and therefore influencing the composition of the capital formation which takes place. In this respect the hoarding of cash balances is one aspect of the more general problem of the deficient or inappropriate financial institutions of underdeveloped countries. In spite of the extremely serious nature of the latter problem, however, it needs to be kept conceptually distinct from the forces determining the volume, as opposed to the composition, of investment activity. Much confusion may result from the failure to maintain a clear distinction between the determinants of the volume of saving, on the one hand, and the factors influencing the supply and availability of financial resources, on the other.[4]

As has already been pointed out, hoarding frequently takes the form of the acquisition of physical commodities such as jewelry, gold, or other durable assets which are regarded as highly liquid and which possess the added advantage over cash balances that they may constitute useful hedges against inflation or political uncertainty. Such preferences obviously involve (in a closed economy) the use of domestic resources in their production, and may therefore be regarded as absorbing scarce productive agents. However, certain structural features and other peculiarities of underdeveloped economies may play an important role in determining the consequences of such preferences. An increased propensity to hoard commodities whose domestic elasticity of production is very low (as seems to be the case, for example, with gold mining in India) will result almost exclusively in a price effect and virtually no output effect; it will not significantly increase the quantity of the commodities hoarded, nor will it result in an increased use of domestic resources for such purposes. There may, then, be sharply defined limits to the extent to which a preference for hoarding physical goods results in an absorption of domestic resources. If, moreover, there are legal (and adequately enforceable) restrictions upon the importation of a commodity whose domestic elasticity of production is very low, the results of an increased preference for such commodities are formally similar to an increased preference for nonreproducible assets.[5]

III. *Preference for Investment in Land*

The classic example of nonreproducible assets is, of course, land. It appears to be a widely held belief that one of the reasons for the low rates of capital formation in underdeveloped countries is that upper-income groups employ their savings in the purchase of land. The following statement by Meier and Baldwin [7, pp. 307-8] is illustrative:[6]

> . . . inequality in the distribution of income does not contribute as much to productive investment as might be expected . . . (because) the group

[4] See [2, Ch. 10, esp. pp. 197-203] for an example of such confusion.

[5] Where such commodities may be legally imported (or where they are successfully smuggled into the country in spite of import restrictions) the result is a drain upon domestic resources because of the increased exports ultimately resulting from such transactions. The effect is the same as if people were acquiring foreign currency or other foreign assets.

[6] Cf. also [6, p. 227] [12, p. 22].

at the top of the income pyramid is composed of land-owners and traders who tend to invest in more land, real estate speculation, capital flights, or inventory accumulation rather than long-term industrial investments or public utilities.

Assuming that the purchase of land was financed out of current incomes, such individual savings must have resulted in the release of real resources for nonconsumption purposes. In so far as a desire for the acquisition of land generates a willingness to abstain from current consumption, it thereby increases the volume of *ex ante* saving, and may, indeed, be one of the most important motives for such saving in underdeveloped countries. If we assume that net saving is positive (i.e., that the savings of some individuals are not offset by the dissavings of others) and that total output does not decline, an equivalent amount of capital formation of some sort must be taking place within the economy.

A "preference for investment in land," whatever it may do to the price of land and the distribution of landholdings, does not, *in itself*, reduce the volume of capital formation by "absorbing" a part of current saving. Nevertheless, the belief that it does so seems to be firmly entrenched in the current literature. It is true, of course, that from the point of view of the individual investor, such a purchase represents a real alternative to the purchase (and creation) of a new capital good. However, the analysis ought not to be terminated, as it typically is, with the purchase of an existing, as opposed to a new, asset. The ultimate impact of the transaction will depend, among other things, upon how the seller chooses to dispose of the revenue received from the sale of his asset. If, as is at least conceivable, the seller of land intends to use the proceeds to acquire a new, reproducible capital good, the ultimate result will be an addition to the community's stock of such goods. It is only where the saving of the buyer is offset by an equivalent amount of dissaving on the part of the seller out of such revenue that real saving (and capital formation) are not increased. Although this is a possible consequence, there seem to be no compelling reasons for regarding it as a necessary consequence of land purchases.

Although a strong preference for land need not, as is commonly asserted, automatically reduce real saving and capital formation, it may have this as well as other important consequences, under certain special circumstances and via a more circuitous mechanism than has yet been specified. For example, in agricultural economies where land is a highly liquid asset, the ease of borrowing, on the part of landowners, by using their land as collateral, may induce a higher level of consumption expenditure and/or the growth of debt for strictly consumption purposes on the part of the owners of land [14, p. 85] [9, p. 69]. In such a case aggregate saving will be reduced, not because of the preference for land per se, but rather because such a preference makes possible an upward shift in the consumption expenditures of landowners.[7]

[7] This, of course, may cut both ways. The high liquidity attaching to land may facilitate borrowing on the part of landowners for investment rather than consumption. All that is suggested here is a set of circumstances under which a strong attachment to land *may* reduce capital formation.

Some such process may, in the past, have played a significant role in reducing aggregate saving in peasant economies. If, to the generally low level of real incomes of small landowners is added the pressures resulting from population growth, occasional crop failure and a variety of other emergency (as well as ceremonial) occasions, the high liquidity uniquely attaching to land may have been the strategic factor making possible the growth of rural indebtedness for strictly consumption purposes.[8]

Moreover, if the rate of return on money-lending to an impoverished peasantry, who borrow on the strength of their small landholdings, is very high (and there is much evidence that this is so) we have an important factor accounting for the low level of investment in industrial enterprises in underdeveloped countries. In addition to such well-known deterrents as high degree of risk, limited markets, and absence of external economies attaching to industrial investment in underdeveloped countries, is the decisive consideration that rates of return for rural lending, even after discounting for risk, are extremely high, partly for institutional reasons and partly because of serious market imperfections.[9]

A further possible consequence of a strong preference for holding land is that it may reduce industrial investment by establishing high interest rates on borrowed funds which act as a deterrent to potential industrial entrepreneurs. A strong preference for land means that owners of wealth (potential lenders) can earn high rates of return by lending money for the purchase of land (i.e., buying mortgages). The combination of a small and inelastic supply of funds with a relatively high demand for their use serves to establish unusually high rates of interest. Therefore potential borrowers for industrial investment find that they must compete, in borrowing funds, with people whose preference for land is such that they are willing to pay extremely high rates of interest for such funds. In this respect a strong preference for land acts as a serious deterrent to industrial investment.

This last case is similar to the one which Keynes seems to have regarded as of major importance [4, p. 241]:

> . . . it is conceivable that there have been occasions in history in which the desire to hold land has played the same role in keeping up the rate of interest at too high a level which money has played in recent times. . . . The high rates of interest from mortgages on land, often exceeding the probable net yield from cultivating the land, have been a familiar feature of many agricultural economies . . . in earlier social organizations where long-term bonds in the modern sense were non-existent, the competition of a high interest-rate on mortgages may well have had the same effect in retarding the growth of wealth from current investment in newly produced capital-assets, as high interest rates on long-term debts have had in more recent times.

[8] It is also possible that the bidding up of the price of land may induce owners of land to increase their consumption expenditures or to dissave because of the increase in the market value of their asset.

[9] Evidence on the profitability of rural lending is available from many sources. Among recent works [10, esp. Vol. II, Ch. 14] and [13] will be found very illuminating.

Thus, statements to the effect that a preference for land on the part of upper-income groups automatically reduces real saving and investment are, at worst, incorrect and, at best, highly elliptic. Where saving and investment are in fact reduced, it is due not to the preference for land by itself, but because of the presence of other important conditions which are often unspecified.

IV. *Inadequacy of Entrepreneurial Talents*

Much attention has been devoted, in recent years, to the apparent shortage of entrepreneurship in underdeveloped countries. It has become fashionable to deplore the absence of Schumpeterian entrepreneurs, and many economists have dwelt at length upon political, sociological and historical explanations for this shortage. Although such noneconomic explanations are not to be despised, it is difficult to avoid the conclusion that these people have been led into a faulty line of analysis by excessive preoccupation with their own definitions. Having defined entrepreneurs as those daring and imaginative individuals who undertake risky long-term industrial investments, the observed absence of such investment in underdeveloped countries is then attributed to the absence of entrepreneurs. A more plausible and purely economic explanation, or at least a more fruitful working hypothesis, is that most underdeveloped countries possess a reasonable number of people with entrepreneurial talents, but that these people behave in a subjectively rational fashion when they adopt short economic horizons and avoid long-term commitments in industry. Many of the activities referred to in this paper in fact help to account for the often-lamented "absence" of entrepreneurship in underdeveloped countries.

To quote from an interesting paper by Henry Aubrey:

> The acquisition of real estate is often considered as evidence of sentimental attachment to land or of feudal patterns of unproductive investment. This explanation may be perfectly correct in some instances; in others, however, such "investment" may result from preferences well founded in the expectation of profit or, conversely, of security against a danger of depreciation that might face other forms of asset-holding [1, p. 398].

Rather than infer from the absence of long-term industrial investment the nonexistence of entrepreneurs, it seems more reasonable to infer that potential entrepreneurs, in evaluating alternative opportunities (including the purchase of existing assets), usually conclude that personal income maximization is not to be achieved in long-term industrial activities. In so doing, entrepreneurs are not necessarily irrational or behaving in response to considerations of social status, prestige, family honor, or even necessarily lacking in the "capitalist spirit." Considering the special circumstances of many underdeveloped countries, their decisions may constitute a perfectly rational evaluation of the structure of economic opportunities. The notion that there is an extreme scarcity of entrepreneurship in underdeveloped countries usually involves a failure to distinguish between the aggregate supply of entrepreneurship and its distribution. To employ the terminology recently adapted by Leibenstein from Von Neumann and Morgenstern, potential entrepreneurs are behaving in

un-Schumpeterian but subjectively rational fashion by selecting to play zero-sum rather than positive-sum games. If adherents of the entrepreneurial school of thought reply that such people are not, by *their* definition, entrepreneurs, they must weigh the serious consideration that such businessmen could conform to their definition only by deliberately failing to employ the calculus of profit maximization.

This question of the determinants of investment decisions in underdeveloped countries deserves more attention than it has so far received. It has, unfortunately, often been lost sight of in the current practice of indiscriminately lumping together a highly heterogeneous combination of factors which are presumed to be responsible for low rates of capital formation and unproductive investment patterns in low-income countries. The following statement by Gunnar Myrdal is illustrative:

> It is . . . highly characteristic of all the underdeveloped countries that their business classes are bent upon earning quick profits not by promoting long-term real investment and production but by buying and selling, moneylending, and other *easier ways* of making money, which also often escape taxation. Profits tend to be invested in land, or else hoarded or transferred abroad, when they are not dissipated in a costly display of wealth and social status. There is a low propensity to save and to invest productively in new enterprises [8, pp. 202-3; italics added].

Myrdal's statement seems to imply that the investment pattern which he deplores is somehow connected with certain peculiar features of the "business classes" of underdeveloped countries. Yet, by his own assertion, they are merely pursuing the "easier ways of making money." Surely what is significant in this context is the existence of such opportunities and not the fact that businessmen take advantage of them. Is it at all probable that "westernized" entrepreneurs would behave differently when confronted with the same spectrum of alternatives?

V. *Conclusions*

A theory of economic development (which involves, of course, an analysis of why development does *not* take place as well as why it does) must include, above all, an explanation for the existing structure and distribution of market opportunities in underdeveloped countries. The fundamental question in an *economic* theory of economic development is: Why is the structure of market opportunities in underdeveloped countries of such a nature that it fails to provide the personal incentive for individuals to undertake those activities which appear to be conducive to economic growth? When this question has been adequately answered, many of the other pieces in our puzzle will fall easily into place as dependent, rather than independent, variables. The low amount of *ex post* savings, the widespread preference for the acquisition of assets which fail to enlarge productive capacity, the apparent malallocation of the small volume of resources which are devoted to investment purposes, the "scarcity" of entrepreneurship and the propensity to engage in short-term speculative ventures, all fall, to a substantial degree, within this category. They represent behavior patterns which may reasonably be expected either

to disappear or significantly decline in importance in the face of a drastic outward shift in the marginal efficiency of capital schedule.

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Wages and Interest—A Modern Dissection of Marxian Economic Models: Comment

Paul A. Samuelson's article, "Wages and Interest: A Modern Dissection of Marxian Economic Models," which appeared in this *Review* (December 1957), though enlightening and interesting, is open to criticism on the ground that it does not closely enough represent Marx's formulation of the issues treated.

1. *Samuelson's Conception of the Marxian Rate of Profit.* Samuelson writes (pp. 886-87):

> Though prices and wages are constant . . . this does not mean that production is timeless or that intermediate products just now produced by labor and machines will exchange one for one against themselves when

> "ripened" one period from now—or one for one against finished goods produced today from last period's inputs.

According to Marx, however, intermediate products will exchange one for one against *themselves* when ripened—if they contain the same quantity of labor; and if they so exchange among themselves, they must contain the same quantity of labor.[1] Time, in the sense of a process of "ripening," is nowhere found in Marxian price theory. Samuelson, however, suggests the presence, as a rule, of such a process in Marx's model. He writes (p. 887):

> Only under special, and unrealistic, market assumptions can the competitive supply and demand relations be expected to ignore these timing relations. . . .

He develops Marx's rate of profit on the basis of the difference between ripened and unripened products (p. 887):

> The fundamental factor relating unripened product today to ripened product one period from now is the market interest rate r (or what Ricardo and Marx would call the rate of profit, a pure percentage per period).

This "ripening" time, though present in Samuelson's Marxian model, does not appear in any guise in the model Marx endeavored to use.

2. *Profits and Wages.* Prefacing his discussion on the incompatibility of falling profits and falling real wages, Samuelson writes (p. 892):

> . . . we must tackle directly the question of what accumulation will tend to do to r, basing ourselves on the actual behavior equations of competitive capitalism.

But the question arises: do Samuelson's "actual" behavior equations accurately represent the behavior equations employed by Marx?[2] The answer seems to be in the negative. The contrast between Marx's and Samuelson's equations stand out in the latter's formulation of a new theorem respecting technological change under perfect competition.

[1] Samuelson uses the following symbols: Y and K, output in consumer and capital goods industries; (a_1,b_1) (a_2,b_2), labor and capital coefficients of production in capital and consumer goods industries; p_1,p_2, cost of production of capital and consumer goods; L_1,L_2, the quantity of labor employed in capital and consumer goods industries; w, the wage-rate; and r, the rate of profit. Equation (4) represents national income expressed in terms of labor; equation (5) represents the cost of production of consumer and capital goods, and equation (6), the explicit solutions for (5) in terms of a_1,b_1,a_2,b_2,r.

$$(4)\quad Y = \frac{1-b_1}{a_2(1-b_1)+a_1b_2}L \qquad K = \frac{b_2}{a_2(1-b_1)+a_1b_2}L$$

$$(5)\quad p_1 = (wa_1 + p_1b_1)(1+r) \qquad p_2 = (wa_2 + p_1b_2)(1+r)$$

$$(6)\quad \frac{p_1}{w} = \frac{a_1(1+r)}{1-b_1(1+r)} \qquad \frac{p_2}{w} = \frac{a_2(1+r)[1-b_1(1+r)] + a_1(1+r)b_2(1+r)}{1-b_1(1+r)}$$

[2] Marx considered the distinction he drew between the "labor-power" going into production and the "product of labor" resulting from the labor process as his greatest contribution to value theory. Exchange between the commodity labor-power and the commodity product of labor cannot be considered an exchange between *themselves*. They are fundamentally different commodities, composed of different factors of production. It is this difference that *allows* for surplus-value.

Samuelson states, "A technical improvement must be an improvement or it will not be introduced into a perfect-competition market economy . . ." (p. 894). By improvement he means a change resulting in either an increase in real wages or in the rate of profit. Thus, he suggests that a rational capitalist would not employ a technique that depresses r, or, that in a labor-dominated economy, labor would not select a technique that diminishes (w/p_2) real wages.

Marx's market economy, however, differs from Samuelson's. Marx assumes the presence of temporary monopoloid market structures. These structures are a prerequisite to innovation, the means through which greater profits (Marx's "surplus profits") may be earned. It is this possibility of realizing surplus profits (monopoly profits) that induces capitalists to innovate. These surplus profits, however, do not persist. Competition, in the Marxian model, compels capitalists to adopt available innovations, hence supplies increase and their price falls to a lower equilibrium (cost of production) level. As a result surplus profits are eliminated; only surplus value, attained by the "exploitation of labor-power," remains. Since Marx *assumes* that the employment of labor-power decreases with the introduction of new techniques, the amount of surplus value realized *may* be less with the new than with the older technique. So Marx, in Volume III of *Capital*, attempts to resolve the "paradox" or "riddle" of capitalist production, namely that the capitalist's quest for higher profit rates results in diminished rates of profit. Only by assuming temporary monopoly conditions in the market economy does Marx arrive at this seemingly paradoxical conclusion, and its corollary that capitalists do not revert to the old technique of production, even if it had formerly been more rewarding, because the now prevailing (lowered) price no longer permits the higher profits formerly associated with use of the old technique. The new position, though less favorable than the old, still is the best to be had.

Samuelson "proves" the incompatibility of falling profits and falling real wages through recourse to the new theorem. First, he sets down the reciprocal of his equation (6) to show that, with specified values for a_1, a_2 and b_1, b_2 (the labor and capital coefficients), a decline in the rate of profit (r) must produce an increase in real wages (w/p_2):

$$\frac{w}{p_2} = \frac{1 - b_1(1 + r)}{a_2(1 + r)[1 - b_1(1 + r)] + a_1(1 + r)b_2(1 + r)}$$

However, as Samuelson notes, Marx, by introducing technological change, makes necessary a relaxation of the assumption of fixed a_1, a_2 and b_1, b_2. Yet, even with changing coefficients, Samuelson asserts, the incompatibility continues to prevail (p. 894):

> Remember that in a perfectly competitive market it really doesn't matter who hires whom: so have labor hire "capital," paying the new market rate of interest $r' < r$; then labor could always use the old technology and paying less than r get better than the old real wage.

In Samuelson's example the old technology is assumed to yield the new rate r'. In the Marxian model, however, it is impossible to obtain r' given the

old technology, since r' is a sequel to recourse to the new technique. If both the new and old techniques are available, and uniform prices are assumed, the old technique is no longer profitable, since the price of the product has fallen with the introduction of the new technique. Therefore, in Samuelson's example, if the old technique is used labor is *not* better off; it may even be in a less favorable position. If one incorporates in the above equation the Marxian assumption that r is a function of the state of applied technology one may select certain values for the variables in the equation which permit falling rates of profit and falling real wages to be compatible. If r were determined by some exogenous factor (e.g., time or "ripening") Samuelson's assumption would be legitimate. Such assumptions, however, lie outside the framework of Marxian theory.

3. *Changing Factor Proportions and Prices.* Samuelson describes what may happen to factor prices and factor shares when their relative supplies (or rates of growth) change, given a situation in which the technical coefficients of production are rigidly fixed. He writes (p. 900):

> In this case where capital goods have ceased growing as fast as labor, the rate of profit has risen to become all of the product. So bizarre a result came from the bizarre assumption of fixed coefficients.

On the following page he adds (p. 901):

> The Marxian model with fixed coefficients presents some quite pathological features. For if the attempt to accumulate were to cause physical machines K to grow relative to fixed labor L, the machines would become redundant in supply and their rents would fall immediately to zero.

Marx would have objected to such statements of the matter. In the Marxian model, the growth rates of K and L depend upon relative factor prices. If K increases more rapidly than L (Samuelson's example), wages increase concurrently with the decline in the price of capital. The total effect of the price change, however, is not instantaneous or as great as Samuelson assumes. Price changes of L and K, in the Marxian system, induce qualitative changes in innovations, and these ultimately counteract the initial price changes. For example, suppose that, compatibly with the Marxian model, K becomes superfluous. Capitalists would obviously switch to more capital-using techniques of production. This "deepening" of capital would make K more scarce (increasing the price of K) and L relatively more abundant (decreasing the price of L, i.e., wages). Thus Marx explained the more rapid introduction of machinery into the American than into the English economy (*Capital*, Vol. I, pp. 429-30). Elsewhere he writes:

> A momentary excess of the surplus-capital over the laboring population controlled by it would have a twofold effect. It would, on the one hand, mitigate the conditions, which decimate the offspring of the laboring class and would facilitate marriages among them, by raising wages. *This would tend to increase the laboring population.* On the other hand, it would employ the methods by which relative surplus-value is created (*introduc-*

tion and improvement of machinery) . . . (*Capital*, Vol. III, p. 256; my italics)

More fundamental to this discussion, however, is the question: Did Marx actually hold to a fixed coefficients of production model? Samuelson answers (pp. 906-7):

> Perhaps Karl Marx really had such a technology in mind. Perhaps not . . . he speaks again and again of alternative techniques. While many of these clearly depict technological change in the production function rather than movement within one function, the fact that the old methods are still known along with the new shows that Marx and Ricardo definitely envisage the existence of more than one technique.

Marx was obscure in his discussion of this matter. In Marx's analysis of the rate of profit however, one finds "averaging" of organic compositions of capital in a specific sphere of production—thereby implying the presence of variable coefficients (*Capital*, Vol. III, pp. 182-85). So Samuelson's assumption of fixed coefficients of production is not always warranted.

4. *Concluding Remarks.* Samuelson's exposition attributes assumptions to Marx (e.g., the rate of profit is a function of time; the market economy is characterized by instantaneous price adjustments; foresight is perfect; and coefficients of production are fixed) which differ more or less from those to which Marx seemed to adhere. Samuelson's findings, though quite valid on the premises postulated, are not accurately descriptive of Marx's system.

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Wages and Interest—A Modern Dissection of Marxian Economic Models: Reply

I must assert agreement with the view that my paper on Marxian economic models did not do justice to Marx's own formulations of the issues treated. Nor was it ever intended to undertake such a task, whose extreme difficulty can be illustrated by the following topic.

What did Marx really think would happen to the real wage under capitalism with its alleged falling rate of profit? Some scholars (e.g., Jürgen Kuczynski) believe Marx proved that the real wage would fall and claim by empirical statistical observation to verify this law of immiserization of the proletariat; other scholars (such as Maurice Dobb, perhaps?) seem to argue that, with exceptions, this was Marx's view about competitive capitalism but that it came to be falsified by historical reality primarily because of the growth in political and economic powers of trade unions and the working class; still other scholars (notably Thomas Sowell in the March 1960 issue of this *Review*) argue with considerable persuasiveness that in his major economic writings, Marx did not conclude that the real wage per hour or day would decline under competitive capitalism. I claim no competence or interest in such doctrinal history.

Of the many uses we can make of the past, one—but certainly not the only one—is to reask some of the questions older writers posed and to provide them with answers in terms of modern analytical methods and terminology. The Marx-like or Ricardo-like model I described could be stripped of all proper names and could as well be described thus: a simple model involving labor, unlimited land, producible circulating- and fixed-capital items; and involving competition-consistent technology.

From the present methodological slant, it may not be presumptuous to wonder what would be Marx's comment on my 1957 model and analysis. I suspect with Gottheil that he might disagree often with my reasonings. Although Marx is not here to speak for himself, it is a legitimate question to ask whether my conclusions can stand up to the objections which seem implicit in Marxian reasonings and categories, and I am grateful to Gottheil for having raised specific queries for further consideration.

Query 1. If Marx and I agree it is unthinkable for production not to take time, which is the more appropriate behavior equation of competitive capitalism: mine, in which unripened spring product sells at a discount to finished autumn product (corresponding to a market-determined positive interest or profit rate); or the view quotable from Gottheil, "according to Marx . . . intermediate product will exchange one for one against *themselves* when ripened—if they contain the same quantity of labor . . ."?

That a modern theorist will agree that my position is the one appropriate to competition seems to admit of little doubt.

Query 2. If an improvement in technology becomes known to one or more producers, and even if they realize that after they have used it for some time competitive imitation will deprive them of their temporary "monopoly" profits, is it possible for the new equilibrium to involve a lower interest (or profit) rate *and* a lower real wage rate? My 1957 answer was categorically No: if the innovation is competitively viable, it *must* either raise the real wage, the interest rate, or both. This theorem I hold to be valid whether there are fixed or variable coefficients of production; and I may remind readers that I have no great liking for the usual narrow interpretation of Ricardo and Marx which imputes to them a rigid fixed-coefficient assumption.

Gottheil seems to suggest that if Marx were here today he could *validly* put forward behavior equations other than mine which would negate my theorem. This I bluntly deny. Whether or not factor supplies react to their changing market prices, whether or not technological innovation is induced by factor-price changes so as to counteract initial price changes (as Marx, Hicks, and Fellner have argued), any new equilibrium that is truly characterized by a lower interest rate $r' < r$, must by the same kind of reasoning that makes two plus two equal four, be characterized by a higher real wage $(w/p_2)' > (w/p_2)$; and if in the short or longer run we have the old and the new technologies persisting side by side, then the computed average profit and wage rate cannot both go down.

If Joan Robinson and I are wrong in this contention, we are dead wrong, it not being an issue upon which two contradictory opinions can be legitimately held. And no reasoned defense of the opposing view have I yet seen.

In standing my ground, I hope I am not contradicting Professor Gottheil

or any one else but rather am clarifying the methodological background to the earlier discussion.

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The Pure Theory of International Trade: Comment

In his admirably lucid and comprehensive survey of "The Pure Theory of International Trade" [1] Robert Mundell ends his analysis by a commendable attempt to see how far the results of the simple two-country two-commodity model remain valid in a multicountry multicommodity world. He concludes that in the latter case it is necessary to assume that tax proceeds are spent entirely on domestic goods in order to establish the conclusion that the terms of trade are improved by a uniform import duty, a consumption tax on imports, or a production tax on exports [1, pp. 105–7, esp. n. 30], whereas in the former model this conclusion can be derived on the assumption that tariff proceeds are redistributed and their expenditure divided between the goods in accordance with marginal propensities to consume. The contrast between the results for the two models is curious in view of the well-known Hicksian theorem that a bundle of commodities whose prices change in the same proportion can be treated as a single good; and it turns out that the difference is due to an error in Mundell's treatment of the income effect in his elasticities of demand for imports. When this error is corrected, it appears that there is no difference between the two cases. For brevity, the demonstration is confined to the effect of a uniform import duty.

Mundell shows that, on the assumption that all export goods are gross substitutes (an increase in the price of one country's exports, other prices constant, improves the balance of payments of every other country), a change in one country's policy will definitely improve its terms of trade if it has the effect of worsening the balance of payments (at initial prices) of every other country. The problem is therefore to determine the effect of the policy change on the balance of payments of the typical other country. For a tariff whose proceeds are spent on home goods, Mundell writes this effect as $-\eta_{jO}I_{jO}$, where I_{jO} is the initial quantity imported from country j by country O (which has imposed the tariff). The elasticity η_{jO} is described as "the elasticity of demand for imports (with respect to own price) . . . from country j to country O" [1, p. 106]. If this is understood in the usual sense of the elasticity with respect to the price of j's good, Mundell's expression for the effect of the tariff is clearly erroneous, since the tariff changes the prices of the other countries' exports to country O, with cross-effects on O's imports from j which are not represented in the formula. Mundell has since explained (in correspondence) that he meant η_{jO} to stand for the elasticity with respect to the price of country O's domestic good; the revised definition is assumed in what follows.

Since η_{jO} is positive by the gross-substitutes assumption, country j's balance of payments is necessarily worsened; hence a tariff whose proceeds are spent on domestic goods necessarily improves the terms of trade of the

tariff-imposing country. But if the tariff proceeds are redistributed and spent like marginal income, the effect (at initial prices) of the tariff on country j's balance of payments is $-\eta_{jo}I_{jo}+m_{jo}I_o$, where the second term is the product of country O's marginal propensity to import from country j and the initial quantity of country O's imports and represents the effect of the expenditure of the tax proceeds. The expenditure of the tariff proceeds introduces a positive term to be weighed against the negative term of the former case.

To determine the net effect on country j's balance, it is necessary to break the elasticity term down into its substitution and income components. It is here that Mundell made his mistake, for he writes his income term as $m_{jo}I_{jo}$, which is less than $m_{jo}I_o$, and concludes that the sign of the effect on j's balance is indeterminate. But $m_{jo}I_{jo}$ is the income term for a rise in the price of the j'th good only, whereas the tariff has raised the price of all imports relative to the domestic good, so that the income term should be $m_{jo}I_o$. This exactly cancels out the effect of the expenditure of redistributed tariff revenue, leaving the effect on country j's balance as $-\eta'_{jo}I_{jo}$ (the prime indicating a compensated elasticity); this is necessarily negative by the gross substitutes assumption,[1] so that j's balance (at initial prices) is worsened and the equilibrium terms of trade of the tariff-imposing country necessarily improved. The effect of a tariff whose proceeds are redistributed is therefore the same in the many-country many-good case as in the two-country two-good case.[2]

In conclusion it may be helpful to make explicit three possibly important points implicit in Mundell's argument. A good may be a "Giffen good" in aggregate demand without being inferior in any individual's consumption [1, p. 75]. The necessity of an inelastic foreign demand if growth is to reduce real income is limited to the complete-specialization model [1, p. 85]. When domestic demand for imports is inelastic a tariff whose expenditure is biased towards imports may turn the imposing country's terms of trade against it [1, p. 86 and *passim*].

HARRY G. JOHNSON*

REFERENCE

1. R. A. MUNDELL, "The Pure Theory of International Trade," *Am. Econ. Rev.*, March 1960, *50*, 67–110.

[1] Gross substitution means that the positive income effect of a fall in the price of one good on the quantity of the others demanded is always outweighed by a larger negative income effect.

[2] In terms of Mundell's formula [1, p. 106, n. 30] the final effect of a tariff whose proceeds are redistributed is:

$$\frac{dP_i}{dt_0} = \sum_{j=1}^{n} \eta'_{jo}I_{jo}\frac{\Delta_{ji}}{\Delta}$$

where the prime denotes compensated elasticity and the determinant ratios refer to the effects of price changes in multiple markets. The compensated elasticities are positive and the cofactor ratios negative under the gross substitutes assumption, so that the tariff unambiguously improves the terms of trade of the tariff-imposing country.

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Inflation: Correction and Restatement

In a paper on inflation in the March 1960 issue of this *Review*, I attempted to abstract from monetary complications by assuming ". . . that the monetary authority provides sufficient funds to meet the requirements of the economy at a fixed interest rate" [4, p. 20]. It has been pointed out to me[1] that this assumption invalidates that part of my argument which considers the impact of a wage-push under the assumption of spender real-income consciousness. It is also inconsistent, in spirit at least, with my assumption [4, p. 29] that "It appears strategic, however, to abstract here from the effects of fiscal policy designed to maintain full employment, in attempting to assess the impact of cost-push, since fiscal policy is a tool used to deal with the unemployment . . . which results from a wage rate increase."

I attempted to show that under certain conditions a spontaneous wage increase, in contrast to a spontaneous increment to demand, could be identified by the unemployment it causes. A spontaneous wage increase tends to generate unemployment under a number of circumstances, three of which are: (1) reduced spending (in real terms) via the money illusion[2]; (2) if the monetary authority does not provide sufficient funds to meet the requirements of the economy at a fixed interest rate, spending may be reduced also via (a) real-balance effects on consumption and (b) a reduction in investment as the interest rate rises[3]; (3) reduction in spending on investment and exports due to rising wage-cost effects.[4] In my formal model, I let the wage increase generate unemployment through the money illusion and then attempted to ascertain the extent to which the resulting shift in distribution of income between wages and profits affected spending and thereby offset the unemployment effects of the wage increase. I concluded that some unemployment was likely to remain. In considering the significance for my results of assuming real-income consciousness, I concluded [4, p. 36] that the unemployment effects would be ". . . offset in whole or in part. . . ." In terms of my assumptions, this was an error—"offset completely" would have been the correct statement. Given real-income consciousness, price flexibility, and adjustments in the money supply to keep the interest rate constant, a spontaneous wage increase would set off a chain of events ending in a new equilibrium in which all factor incomes and prices would have increased proportionally leaving the real variables of the system, including the level of employment, unaltered.[5] Under these circumstances there would be no redistributive effects.[6]

[1] I am very much indebted to Michael Lovell and my colleagues J. Crutchfield, D. Gordon, and A. Zellner for discussions of the contents of this note.

[2] Defined here as "money price illusion" [3]. "Money income illusion" could lead to increased employment.

[3] Assuming no liquidity trap and an investment function not perfectly inelastic with respect to the interest rate.

[4] These so-called "indirect cost-pull" effects were considered in the paper but not incorporated into the part of the model under discussion.

[5] Unemployment could of course occur in the transition to the new equilibrium.

[6] This follows well-known propositions advanced by Walras, Hicks, and Lange (cf.

Had I assumed an incomplete adjustment of the money supply to the rising wage and price level, real-balance effects would have reduced consumption expenditures; in addition, a rise in the interest rate would have curtailed investment spending. Under these circumstances, the shift from money illusion to real-income consciousness would not have completely wiped out the unemployment generated by the wage-push.[7] Obviously, a wage-push with both money illusion and monetary stringency would induce a higher level of unemployment than with money illusion alone and for this reason would be easier to distinguish from a demand-pull situation (which inevitably leads to reduced unemployment).

Spontaneous wage increases may generate inflation and it is meaningful, in my opinion, to try to distinguish such an inflation from a demand-induced inflation. Changes in the level of employment can only be used to distinguish them if the wage-push has co-conspirators like money illusion and incomplete monetary adjustment—the more and the stronger the co-conspirators, the easier the distinction.[8]

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2. MARTIN BRONFENBRENNER, "Reply," *Jour. Pol. Econ.*, Oct. 1957, *65*, 445-47.
3. T. C. LIU AND C. G. CHANG, "Rejoinder," *Am. Econ. Rev.*, March 1953, *43*, 148-51.
4. F. D. HOLZMAN, "Inflation: Cost-Push and Demand-Pull," *Am. Econ. Rev.*, March 1960, *50*, 20-42.
5. K. W. ROTHSCHILD, "Aggregative Wage Theory and Money Illusion," *Jour. Pol. Econ.*, Oct. 1957, *65*, 442-45.

[1, Ch. 6] for a summary statement) and has been demonstrated for a model similar to mine by Rothschild [5].

[7] Bronfenbrenner [2] argues similarly in answer to Rothschild [5].

[8] But, we might add, the quicker the wage inflation will grind to a halt.

BOOK REVIEWS

General Economics; Methodology

Czechoslovak Economic Papers. Prague: Czechoslovak Academy of Science (Ceskoslovenska Akademie Ved), 1959. Pp. 223.

This volume consists of 17 different articles, varying in length from 10 to 20 pages and dealing with a variety of subjects. As a result, the reviewing of such a nondescript collection is not easy, especially since in the judgment of the reviewer, none of these contributions is outstanding. Some of the articles are mere descriptions, while others contain propaganda, and, presumably, attempt to justify current economic policies of the Czechoslovak rulers. The question may be even raised as to why this collection has been translated into English and sent to the American Economic Association.

To indicate the variety of the subjects discussed, the titles of the articles and their authors follow: "International Division of Labor in the World Socialist System," by Vladimir Kaigl; "The Inter-relation of the Expansion in Gross Social Product and National Income," by Felix Oliva; "Certain Problems Connected with Building the Material Production Basis of Socialism," by Pavel Turčan; "Some Thoughts on the Class-Relations in Our Village," by Michal Faltan; "Certain Problems of the Operation of the Law of Value on the World Socialist Market," by Josef Mervart; "The Problems of Economic Efficiency in Foreign Trade," by Villiam Černiansky; "On the Basic Relations in Technical Progress and Their Influence on the Growth in Productivity of Labor," by František Kutta; "Problems of Stable or Variable Wholesale Prices," by Antonín Minář; "Economic Problems of Relations Existing Between Demand and Supply in Socialism and of Planning the Composition of Retail Sales," by Miloslav Kahoutek; "Czechoslovak Cost-of-Living Index New Series," by J. Mach; "Some Conclusions from a Statistical Analysis of the Census of Fixed Production Funds in Engineering," by Jiří Skolka; "The Disaggregation of an Absolute Increment," by Jiří Bouška; "The Evolution of Soviet Views on Statistics," by František Egermayer; "The Role of the State in Contemporary Capitalism," by Jaroslav Langr; "Agriculture in the U.S.A.," by George S. Wheeler; "Effect of the Structure of India's Economy on Her Foreign Trade," by Zdeněk Švejnar; "Emigration from Slovakia Between the Years 1870-1940," by Jan Svetoň.

In view of the variety of topics, there is little reason for reviewing all these articles. However, a few of them may merit some comments. Vladimir Kaigl's article, "International Division of Labor in the World Socialist System," contains a criticism of past and present policies of the countries behind the Iron Curtain in their attempts to reach self-sufficiency, and the resulting low volume of trade between them and with the outside world. He pleads for expanded international trade based on the principle of comparative advantage and ob-

serves that this can be attained by determining more exactly costs of production. He is critical of the theory prevailing in these countries, that foreign trade is "a kind of marginal supplement to the national economy used as a first aid in case of sudden developments of disproportions. According to this 'theory,' the purpose of foreign trade is to import what we need from abroad and to export as much as necessary to pay for these imports" (p. 19).

Villiam Černiansky's article on "Problems of the Economic Efficiency of Foreign Trade" is a discussion of ways and means to achieve "the greatest possible savings of social labor by exchange of goods with foreign countries." The point is made that "a simple comparison of domestic prices with the price obtained abroad is not satisfactory as domestic prices are notoriously distorted by too great differences in the mark-up and taxes and by inclusions in the price of components that ought not be included at all" (p. 111).

"The Role of the State in Contemporary Capitalism" by Jaroslav Langr is a typical Communist diatribe against the Western world, with the usual humbug about capitalist evils and inequities. The article concludes that "after World War II capitalism has not entered any higher evolutionary stage modifying in any way the nature of imperialism, as the highest stage of capitalism. After World War II, the tendencies of the state monopoly capitalism had gained momentum primarily through the militarization of the national economy, through nationalization of certain industrial enterprises, through a more intensive use made of State budgets. This evolution did not result, however, in any changes marking the beginning of a new stage of imperialism. The evolution is taking its course in accordance with economic laws and together with the strengthened tendencies of State monopoly capitalism provides further evidence of the disintegration of this social system and the creation of a material basis for a new and higher social order—that of socialism" (p. 288). In the article "Effect of the Structure of India's Economy on Her Foreign Trade," Zdeněk Švejnar concludes that as a result of British colonial policies, India, since achieving independence, is at the mercy of the United Kingdom, the United States and the German Federal Republic regarding her imports and exports. An expansion of India's trade relations with the countries of the Socialist bloc is necessary to accelerate economic expansion and independence according to him.

Most of the authors refer to the sacred writings of Marx, Lenin, and Stalin in support of their assertions.

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Basic Economics. By THOMAS J. HAILSTONES. Cincinnati: South-Western Publishing Co., 1960. Pp. iv, 513.

Basic Economics, written for a one-semester course populated by nonmajors, is devoted exclusively to macroeconomics. Hailstones writes in crisp short sentences, pitching his analysis well below the average elementary text, including many tedious repetitions and numerical illustrations, with an occasional humorous aside. Bearing many similarities to a handbook, emphasizing

definitions, techniques, and statistics, the book will do little to stimulate its audience to do further thinking and reading about economics. An offsetting factor is the questions at the end of each chapter, raising interesting problems relating economics to other disciplines, but requiring analysis beyond that offered in preceding pages. A short book must be selective, and this one has chosen for major attention: business cycles and money and banking.

The first four chapters cover the usual introductory material clearly, but too briefly for the reviewer's taste. Even though Hailstones' objective is to expound macroeconomics, more could have been done to set the scene in terms of scarcity and allocation. The novice is not given a clear picture of what is excluded and how this limits the analysis. And since he aims at people who will not again brush against formal economics, the relation of other scientific thought to the problems encountered and methodology adopted could have been more fully explored.

Part II, perhaps the best in the book, describes the banking system and contains a thorough description of money creation. One might quibble about presenting the transactions analysis before defining money, and question why general credit controls are alleged to restrict money for specific uses (p. 148) with no analysis of rationing by interest rates. An unusual amount of space is devoted to the history of banking and the structure and techniques of the Federal Reserve System, with only a few pages given to an evaluation of the effectiveness of monetary policy. Inflation, regardless of degree, is painted as an unmixed evil.

The following Part is a potpouri of definitions and miscellaneous relationships, called "Production, Employment, and Income," including an overview of the determinants of GNP and a balanced but dated discussion of the Employment Act of 1946. Its chief merits are a good description of the limitations of GNP as a measure of well-being, an interesting discussion of the prediction of the GNP, and a useful short exposition of flow-of-funds accounts. A mass of statistics depicting personal distribution of income is offered without reference to the functional theories listed, but left uncoordinated, in Part I. Lack of a clear statement on the consumption function leads to a confused, and partially inaccurate, definition of the multiplier (pp. 225-30). Nevertheless, the stage is appropriately set for the second half of the book: instability, the malady and its treatment.

The first two chapters of Part IV present an appropriate general discussion of fluctuations, perhaps a bit too simplified, but including a few novel devices to enhance the clarity. The rest of the section describes all of the traditional business cycle theories, but does little about integrating them.

The final Part portrays income-expenditure analysis first in terms of aggregate demand and supply and then in terms of the consumption function and marginal efficiency of capital. The first approach is quite successful but the second suffers from a failure to distinguish between planned and realized investment. This section also includes a fair but conservative evaluation of policies for dealing with price and employment stability, but neglects growth implications.

Hailstones' writing is neither provocative nor muddy; hence the book could

be advantageously used along with "controversial" material. However, any rigorousness in analysis would have to be provided by the instructor.

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Concepts and Cases in Economic Analysis. By AARON W. WARNER and VICTOR R. FUCHS. New York: Harcourt, Brace and Co., 1958. Pp. xv, 288. $3.00.

Professors Warner and Fuchs of Columbia University have produced a supplementary case book for use in a one- or two-semester introductory course in economics. In content, about one-fourth of the book is devoted to discussion of analytical tools and concepts, with the remaining three-fourths devoted to cases. There are many (188) short cases. Each case is followed by several questions designed to apply the principles outlined in the preceding expository sections. Some cases presuppose a knowledge of such economic institutions as the banking system, the elements of business organization, and trade unions, and of the procedures of national income accounting, not covered in the expository section of the book itself. For this reason it is necessary either to build on a prior course in economic institutions and principles or to use another book of readings or standard text concurrently.

In its organization, this book is heavily weighted in microeconomics. Of its total 284 pages, 196 pages or 69 per cent of the content is devoted to price theory. Only 58 pages or 20 per cent is devoted to aggregative analysis. Of these 58 pages, 15 are devoted to monetary concepts, and the balance to Keynesian concepts. Some major areas, including international trade, labor economics, corporation finance, and distribution theory, are discussed only incidentally or not at all.

Use of this book in an introductory course in economics, like any other case book, will add some variety to an introductory course, perhaps at the expense of breadth and institutional description. This particular case book would also add a great deal of interest to the course. The cases are almost universally interesting and well selected. Many of them are from the world of the *New York Times,* the *Wall Street Journal,* and semipopular magazines. One attempt to draw from the commonplace may have gone too far. A case entitled "Branch Rickey's Baseball Equation" requires for its analysis a more thorough knowledge of baseball than of economic principles.

The questions for thought and discussion following each case are particularly well done. The questions bring out important economic principles illustrated by the case and provide the student with exercise in some analytical tools. For many students study will become more productive as a result of the forced thinking involved in answering the questions. Possibly also the class period will be enlivened by debate.

Along with the above strengths attributed to the book, there are also weaknesses. The most obvious is its rather lopsided organization, with considerably more than half of the book devoted to price and output problems. There are far more cases included in this section than a one-year course could

profitably digest. The area of money and banking in contrast is given very little attention. Only one of the many important analytical problems of the field is considered—the forces determining the general price level are analyzed through the equation of exchange. Only about two pages are devoted to the important area of monetary policy. International economics is given attention in only seven cases, and most of these analyze elasticity of demand for imports. There are no cases concerned with corporation finance, though a few cases are included dealing with break-even charts, refusal pricing, and marketing problems. For these reasons, the book does not adequately meet a need for cases covering the full range of interests included in standard introductory courses. In their preface the authors state that "We have designed the book to be used either as a basic text in economic analysis or as a supplement to be used with other readings." For this purpose, a more balanced treatment would seem in order.

For limited rather than general use this reviewer believes that this case book will make a useful and important contribution to our teaching aids. Its adoption by individual professors will, however, depend upon the degree to which one wishes to emphasize (1) price theory and secondarily Keynesian theory rather than a broader range of topics and (2) economic analysis rather than institutional description.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Ekonomicheskii raschet nailuchshego ispol'zovaniia resursov. (Economic Calculation of the Optimum Use of Resources.) By L. V. KANTOROVICH. Moscow, 1959. Pp. 343.

In a pamphlet published in 1939, Kantorovich gave a general formulation of the linear programming problem, and described a practical method for solving such problems. His method was based on finding what he called "resolving multipliers," which a Western economist would today recognize as the shadow prices of the dual of the linear programming problem. For nearly twenty years this pioneer work was ignored by the Russians, but with the interest in mathematical methods in economics since Stalin's death, they have "discovered" it, and economic thinking in the direction pursued by Kantorovich has been approved and encouraged. The book under review is one of the results. In this book Kantorovich has developed his ideas far beyond the point achieved in the original article. He now recognizes the broader significance of his resolving multipliers and acknowledges their meaning by labeling them "objectively determined values." Much of his book is devoted to the demonstration that the finding of these values is equivalent to finding the optimum plan, that is the plan which gives the largest output. In the course of the exposition he develops the concepts of opportunity costs, the rental value of

superior land, quasirents on capital goods, and scarcity values for current inputs. In Chapter 3 he shows how his methods can clarify some of the issues in planning capital investment. He starts with the proposition that only those capital goods should be created for which the quasirents exceed the cost of production, both figures being based on his "objectively determined values." But there are many more items meeting this criterion than can be created out of current investment resources, and the rule is to create them in descending order of productivity up to the limit of investment resources. He explains the rationale for discounting future quasirents in the process of capitalization and shows how this is to be done in several situations. Finally he lays stress on the point that these objectively determined values are not only an aid in calculation, but will also serve as a set of indexes which, in contrast to existing prices and value indicators, will provide a rational set of incentive indicators and an accurate evaluation of performance, for the guidance and control of local management.

Thus what started out as an algorithm for finding optimum values of certain variables in a problem of production planning has culminated in the rediscovery of much of Western value theory. Of course it is a seriously truncated value theory, which treats the composition of output as already determined and only seeks to maximize output in the proportions assigned. It is still a cost theory of value, but a sophisticated one, involving imputation and opportunity costs.

This is surely a portentous event in the development of economic thought in the Soviet Union. It is a significant step in freeing the Russians from the limitations of Marxist theory, so that Soviet economists can at last provide some clarification of the issues that practical planners must deal with. It marks the way for the transformation of Soviet economists from priests to scientists. Whether these potentialities are realized depends on whether this threat to orthodoxy is allowed to pass. Kantorovich's objectively determined values and Marxist value are clearly not the same thing, and Kantorovich has not been unaware of this problem. He has put probably the best possible face on the matter. He contends that there is really no contradiction, basing his argument mostly on the idea that "socially necessary labor time" under modern conditions of production acquires definite meaning only when one considers alternatives, capacity limitations, and current availabilities, and that all he has done is to work this out concretely. He has taken care not to make the politically dangerous step of formulating his value theory in a way which might imply that composition of output was a problem to be solved. Likewise he avoids the ideological *gaffe* of bringing in utility (a concept inevitably linked with the epithet "notorious" in the Soviet discussions of value theory). His values *are* objectively determined by all the material circumstances of the problem to be solved, and he has forestalled any taint of idealism. But others have not been willing to ignore the discrepancy; and both in the introduction to the book by Nemchinov and in reviews in the economics journals this discrepancy is noted. But the book is published (though in a relatively small edition), and the defense of orthodoxy put up by Nemchinov and the reviewers can only be described as lackadaisical. Any perceptive sophomore can see that

Kantorovich's values are not Marxist values, but that they are indeed "objectively determined," that is "true" values. But apart from its fate as a foray into the theory of value, the book is a testimony to the proposition that the problem of economic theory is to show how to maximize the output from scarce resources, and this portion of its message alone will mean a revolution in Soviet economic thinking.

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Capital, Interest and Profits. By B. S. KEIRSTEAD. New York: John Wiley and Sons, 1959. Pp. 180. $4.00.

This book is at once pleasing and provocative and a scholarly blending of theory and history, aggregative and partial analysis, and the economics of developed and underdeveloped countries. It presents criticism of "accepted" theories as well as original contributions, all this within 171 pages. The book is divided in two equal parts.

The analysis of the first part concerns the process of capital formation. This requires investment (the making of tools) and saving (the provision of purchasing power). Capital formation is unlikely to get started without a "social shock," which initiates the investment and provides the necessary saving. The process takes time and there is a lag between the start of the investment and the resulting increase of output.

This sketch of capital formation is the background for Keirstead's criticism of the marginal productivity theory of distribution and the time preference theory of interest and for his theory of profit, interest, and return to stock. His theory of profit stresses the uncertainties of innovation as contrasted to risk. The uncertainties arise from two kinds of expectations: general, those arising from forecasting the gross national product, price levels, etc.; and particular, those pertaining to the expectations for the industry or firm. In a static economy with perfect knowledge and competition, profits would not exist.

Keirstead presents the loanable funds theory of interest. The rate which firms have to pay for investable funds must cover the opportunity cost of consumer borrowing including that of government, and in addition a return to cover the costs and risks of the lender. Interest is not a reward for waiting because a large proportion of saving is supplied involuntarily by the rich and by corporations, and through monetary and fiscal policies. Only a small proportion of saving is supplied voluntarily and this supply moves inversely to changes in the rate of interest. Hence the rate reflects a large element of scarcity value under normal circumstances.

The analysis then turns from the general market for capital to that of the firm. Keirstead rejects the marginal productivity theory that the firm equates the marginal cost and marginal revenue of capital. Nevertheless he presents a determinate explanation of the firm's use of capital. On the supply side the firm must pay the opportunity costs of the different sources of funds which it must have to carry out its plans. These are its own internal funds, bank loans and the flotation of new securities. The opportunity cost rises as the firm has

to draw upon additional sources so that the firm is confronted by a "stepped" supply schedule.

On the demand side the firm's schedule is likewise discontinuous. The investment plans of the firm are governed by three factors in addition to the expectation of gain: flexibility, economies of scale, and the time horizon. Investment plans are based upon a variety of hypotheses and should be flexible since expectations may turn out to be incorrect. Economies of scale together with the time horizon of the plans determine the growth of the firm. The demand schedule of the firm slopes downward because, other things being equal, uncertainties increase with the length of the time horizon. Hence the firm's use of capital is determinate.

In the conclusion to Part I Keirstead writes, "What I have tried to do in this brief essay is to supplement criticism which I have made in previous books, of the marginal productivity theory of distribution with some more positive statements" (p. 80).

Part II is devoted to "the elaboration, illustration or application of points made briefly in passing in Part I" (p. ix) and consists of five essays. The first illustrates the characteristics of a "conventional" economy and the conditions that are necessary for the initiation of capital formation with a wealth of historical and anthropological examples. The second is a case study of Newfoundland. The third is an application of sector analysis in theory and practice to the dual problem of inflation and unemployment. The fourth essay is a brief criticism of certain cyclical concepts, periodicity, the multiplier, and the acceleration principle. The last is a brief conclusion to Part II followed by a statement of implications for policy.

This is a provocative little book both for what it says and for what it does not say. Its pentrating criticisms induce reactions both pro and con. Here I wish to offer some comments on what it does not say; for my criticism is that the book is too short. In the preface, again in Part I and at the end of Part II the author states that the book is concerned with the theory of distribution. But Part II deals mainly with problems of capital formation. The average undergraduate is unlikely to integrate the two parts for himself. Of more importance for the advanced reader is my impression that the truly original argument concerning the firm's demand for investment might have been more fully developed. This is new ground and needs cultivation.

Notwithstanding these comments this short book offers rich rewards for a variety of students and because of its unique scope and stimulating style should receive a wide acceptance.

JOSHUA C. HUBBARD
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Studies in the Economics of Welfare Maximization. By P. R. BRAHMANANDA. Bombay: Bombay University Press, 1959. Pp. xiv, 520. Rs 18.

Dr. Brahmananda begins his preface with: "By and large economics has been pursued not merely for the sake of knowledge but also for the sake of the healing and hope that knowledge brings" (p. viii). The whole volume reflects this point of view. The author has read and interpreted the classics with

sympathy and understanding; but his primary interest has been not in theory qua theory, but in the tools that theory has forged which may be used in dealing with problems of economic development.

Part I traces the evolution of thought in the writings of Smith, Malthus, Ricardo, Mill, and Sidgwick on the role of the state, conditions favorable to enterprise, and the factors bearing upon population change and capital formation. Brahmananda notes (and the reviewer approves) that "the present antithesis between growth and allocation is not justified and to understand the process of change and its determinants we have to study in detail the workings of the allocation mechanism: it is through the latter that the changes in the supplies of factors can be influenced" (p. 158).

Part II begins with an exhaustive discussion of Marshall's analysis of the surpluses that arise, both to consumers and to producers, because of the conjuncture in which individuals find themselves when they trade. The author is aware of the difficulties involved in measuring these surpluses and of the current controversy in this area, but he expresses confidence in the possibilities of quantification for purposes of economic policy, formulation almost to the point of making a fetish out of the idea. For example he writes, "It is now necessary to concern ourself with practical problems. It is here that the concept of consumers' surplus is of immense aid" (p. 253). One wonders just how one would get the necessary data to employ the surplus concept in deciding whether to use Indian resources in building a new steel mill rather than a textile plant.

This section continues with an examination of how value judgments, the stock of knowledge, the distribution of income and wealth, indivisibilities, individual versus collective perspective, and the divergence of private from social product bear upon economic welfare.

Part III catalogues a number of involuntary infra-optimal situations and the need for remedial action. Much use is made of the concept of increasing returns (again displaying the strong Marshallian bias that permeates the entire volume) and of the possibilities of external economies. This part concludes with a chapter on public utility pricing.

Brahmananda is at his best when discussing the conditions necessary to promote economic development in India. For example he observes that "Competition is only the husk; the kernel is the strength of the economic motive" (p. 385). And again he writes, "In some countries, such an action [i.e., deliberate state action] requires a change in time-honoured customs, traditions and mores, against which individual units might be striving in vain; it requires support and encouragement for the limited number of leaders, who are making the community conscious of the non-optimal positions; it requires a coordination of the efforts of all those who are striving to raise the tempo of progress; it requires direct State activity or initiative to break the rigid bonds which keep individual units in chains, as it were. In other words, in most undeveloped countries, a well-coordinated and concerted attack, planned from the vantage point of a collective perspective, may be the only method by which the so-called 'natural' trends and courses of activity, which have such tremendous inhibiting effects, can be transformed" (pp. 430-31).

In the reviewer's judgment, students of welfare economics and of economic development will find much of interest in this volume. It does not break new ground in the area of pure theory but it does contain real insight with respect to some of the most crucial economic problems of our times.

WILLIAM B. PALMER

The University of Michigan

La répartition du revenu national. Vol. 3, *Modèles classiques et marxistes.* By JEAN MARCHAL and JACQUES LECAILLON. Paris: Ed. Génin, 1959. Pp. 393. 2,400 fr.

The volume under review is the third volume of a four-volume study of the distribution of the national income by Professor Marchal of the faculty of Law and Economic Sciences of the University of Paris and Professor Lecaillon of the faculty of Law and Economic Sciences of the University of Lille. The first two volumes were gracefully reviewed in *Review* for September 1959 by George Jaszi. In those volumes the authors divided receivers of income on the basis of five major groups and analyzed the characteristics of these varied recipients.

In Volume 3 Marchal and Lecaillon begin the second and last part of their long work, the part which studies the processes by which these groups obtain their respective portions of the national income. Before attempting the construction of a model of distribution viable for the modern world, the two collaborators deem it wise to investigate the different models proposed in the past century and a half in order to ascertain in what measure these models still provide instruments of analysis acceptable for the comprehension of today's realities. Hence the third volume is devoted to a critical discussion of models, classical and Marxist. Volume 4, in preparation, will take up the neo-classical, Keynesian and post-Keynesian models.

By way of introduction, the Physiocrats are held incontestably to have been the first scholars to have raised the problem of distribution in its entirety and clearly to have employed the marcroeconomic and sociological approach. The classical model is then scrutinized in three chapters of approximately 35 pages. Adam Smith is hailed as the founder of the English classical school but as far more concerned with problems of production than of distribution. The real architect of the classical theory of distribution emerges in the person of David Ricardo. While the approach of Smith is looked upon as necessarily that of microeconomics, the approach of Ricardo, like that of the Physiocrats, is regarded as embodying the essentials of macroeconomics.

Discussion of the Marxist model, by contrast, runs to approximately 335 pages. In five relatively long chapters the reader is introduced to the functional aspect of the Marxist model, the determination of the variables of the model, the evolution of the variables, and the evolution of the different social classes which share in the national revenue. Chapter 6 closes the discussion on a lofty note with some general reflections upon the Marxist model.

By reason of the many ties that allegedly exist between the classical and the Marxist theories, Marx has often been called the spiritual son of Ricardo. Yet Marx went much farther than his predecessor, believing as he did in a

long-range analysis of distribution which definitely classifies his theory as sociological, an approach barely considered by Smith and only slightly elaborated by Ricardo. With Marx, furthermore, the time is surely coming when at long last the total revenue of society will be lodged in the hands of the masses alone—the proletariat. Complete macroeconomics—a term of course unknown to Marx—will then be the order of the day.

Documentation is abundant and at times fascinating. The style is typically Gallic in its felicity. For readers whose command of the French language is fluent, here indeed is rewarding fare.

The issues raised by Jaszi in his review of the earlier volumes, however, still remain unsolved. Nor can any fair evaluation of the ambitious study undertaken by Marchal and Lecaillon be made until the promised fourth volume is at hand. This reviewer, mindful of the pleasure and profit which a careful perusal of the third volume has accorded him, awaits with anticipation the publication of the final volume in which the neoclassical and contemporary models of distribution are to be dealt with.

JOHN M. FERGUSON

Muhlenberg College

The Wage-Price Issue—A Theoretical Analysis. By WILLIAM G. BOWEN. Princeton: Princeton University Press, 1960. Pp. xv, 447. $8.50.

This book examines the question whether "the wage- and price-setting institutions of the contemporary American economy impart an upward bias to the price level." Of the various processes alleged to lead to such an upward bias, the one selected for detailed analysis is the "dilemma model" which provides the framework for much of the current discussion of this question. The model may be summarized in three assertions: (1) in our society strong unions are able to raise wages faster than productivity in general can advance; (2) these increases in unit cost of labor and unit total cost of product encourage producers to raise prices; and (3) monetary authorities are then faced with the dilemma: (a) to increase the supply of money, thus justifying the higher price level and setting the stage for another round of wage and price increases, or (b) to refuse to increase the money supply, thus halting the inflationary trend but causing unemployment by denying the expansion of funds necessary to buy all of the products available at the higher prices. These three assertions are analyzed in great detail in three separate sections which comprise the main body of the book.

The author finds that all three assertions contain serious faults which make the assertions highly suspect as a basis for public policy decisions. He argues that the wage-determination assertion rests upon a serious oversimplification of the complex relationship between the institutional factors of trade-union structure and the collective bargaining process, on the one hand, and the economic determinants of labor productivity, on the other. The reviewer believes that the author succeeds very well in showing that the wage-determination assertion is misleading and also that it is unsatisfactory because it fails to comprehend the basic causes of wage change.

His analysis of the cost-and-price-determination assertions begins with the

impact of wage adjustments on costs and proceeds to the impact of costs on prices. The author points out that the close link which the assertion assumes between unit labor costs and total costs of product is in reality quite loose unless certain possibilities, such as the substitution of other factors for labor, the change in the costs of nonlabor factors, and the alteration of the rate of output, can be eliminated from consideration.

The same kind of conclusion is reached with regard to the impact of unit costs on prices. The strict cost-plus pricing doctrine of the dilemma model does not give an adequate answer, since it is difficult to apply to sectors of the economy such as the administered price sector. Also the general price level depends upon innumerable individual price decisions, and demand considerations cannot be neglected. But the excess demand model does not provide a useful framework either, since it is applicable only to the competitive section of the economy. The author, therefore, attempts a fusion of the two to give a more serviceable approach.

The discussion of the monetary-policy assertion leads to the conclusion that it is incomplete, because it does not explain how monetary policy affects wage, price, and employment decisions. The importance of the monetary environment should not be underestimated, but the purely monetary approach does not answer certain fundamental questions such as the effect of change in velocity and in money supply, and the influence of the factors which determine private decisions to spend. Questions arise concerning the policies which the monetary-fiscal authorities are likely to adopt when the price level is being pushed upward, and what the impact of these policies will be. The inadequacies of the monetary-policy assertion add to the weaknesses of the dilemma model.

In spite of its faults, the author feels that the dilemma model has served a useful purpose in focusing attention on the macroeconomic problem, and that the controversy over the dilemma model has encouraged consideration of the relative desirability of alternative public policies before adopting any specific program. He concludes with the thought that computation of the relative costs of the various alternatives is likely to be arduous and time-consuming, but that to date no other acceptable procedure has been developed.

The author has covered his subject thoroughly and has defined his terms carefully. His analysis of the very difficult problems involved is painstaking and detailed. He is examining highly controversial matters, and disagreement with some aspects of his work is to be expected. But he has tried to account for every possibility, and although the result is wordy in places, the book is well organized with frequent summaries, comments, conclusions, and helpful diagrams to keep the reader on the track. Only an occasional slip was noted, such as the apparent confusion on page 29 of buyers' inflation with cost inflation and sellers' inflation with demand inflation. Good use has been made of the available literature in the field, and the book contains an excellent bibliography.

PAUL L. KLEINSORGE

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Money and Income. By A. C. L. DAY and STERIE T. BEZA. New York: Oxford University Press, 1960. Pp. xv, 634. $7.50.

As a systematic exposition of macroeconomic theory for both a closed and open economy, this book is probably superior to any undergraduate text currently available on the U. S. market. Based on a volume originally prepared by the senior author for British consumption it shows the felicity of expression that one has learned to expect of British authorship.[1] It is truly a literary work depending on the skillful use of language to persuade the reader rather than on copious statistical tables or their graphic representation. Discussions of suggested readings after each chapter guide the reader into the standard works and the most recent periodical literature. A mathematical appendix is provided explaining the derivation of the various algebraic formulas provided throughout the text. These characteristics together with the level of abstraction attained throughout suggest that students of economics in English universities in their second and third years (the intended market for the original volume) are made of sterner stuff than their American confreres.

The first two sections are taken up with the determination of income levels and the rate of interest. A simplified monetary economy is the basis of the analysis with only three kinds of transactors—households, firms and banks—and three wealth forms—undated bonds, money and real wealth—being involved. Where the analysis goes beyond conventional treatment is in its discussion of dynamic movements from one equilibrium level to another. Here as in other parts of the book imaginative tables are constructed which trace the sequence of changes in the relevant variables. An analysis of the mutual determination of income and interest rates along the lines of Hicks' famous treatment occupies a first-class chapter in the second section.

Most of the third section, on the present-day institutional framework, is taken up with a description of the commercial banking system and the operations of the Federal Reserve. The role of government in economic activity is elaborated. In a final chapter the authors explain their preference for a liquidity-preference approach to interest determination over a loanable-funds approach.

Up to this point, price level changes have been ignored. This is remedied in the fourth section which develops a theory of inflation. An excellent introductory chapter singles out the weaknesses of the quantity theory down to its recent reformulation by Patinkin. The analysis of inflation is then carried on in the context of the modern theory of income determination. Inflation is seen to be the outcome of an excess of planned demand for goods and services over planned supply at full employment. From this perspective, wage increases are "defensive reactions" to price increases rather than initiating impulses.

In Part V, "Stability and Instability," a theory of the trade cycle is advanced on the basis of the familiar interaction of the acceleration and multiplier principles. The most intriguing chapter summarizes the work of Tustin and Phillips in applying engineering techniques to problems of economic

[1] A. C. L. Day, *Outline of Monetary Economics* (Oxford, Eng., 1957).

policy-making. The final two sections "International Monetary Economics" and "International Monetary Experience" consume about one-third of the contents. The first provides a rigorous discussion of the conditions for external balance using the same methodology of planned and realized values (here payments to and from foreigners) that was employed in the income determination section. The final section tackles the policy problems of simultaneously achieving internal and external economic stability.

Accepting the avowed intention of this book to concentrate on macroanalysis, it has certain notable sins of omission. One finds no discussion of the national income accounts, their conceptual basis, construction and limitations. There is virtually no discussion of consumer credit and other selective credit controls because the authors mistakenly believe that nothing can be said about them in "general terms" (p. 176). One and a half pages on nonbank financial institutions, including a one-sentence parenthetical reference to the possible effects of money-substitutes on the demand for money (pp. 124-25), are an inadequate recognition of the kind of work being done by Gurley-Shaw and Goldsmith.

The book's greatest sin of omission is more of a commentary on the state of economic knowledge than a criticism of the book per se. Except in the most general terms, when the history of U. S. fluctuations since the first world war is being discussed, little attempt is made by the authors to demonstrate empirically the validity of their macrotheory. This is probably explained by our ignorance of the causes of economic instability.[2] But if so, the lucidity of this volume may be a snare and a delusion.

If present efforts to make the money and banking course a course in macrotheory succeed, there should be a substantial market for this book. But its significant gaps in the economics of the financial firm and the mechanics of the financial system suggest that there is still room for a high-level course in money and finance which would precede the course in macrotheory.

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The Business Cycle. By R. C. O. MATTHEWS. Chicago: University of Chicago Press, 1959. Pp. xv, 300. $3.00.

In the manner of its predecessors, this latest edition to the Cambridge Economic Handbooks is "intended to convey to the ordinary reader and to the uninitiated student some conception of the general principles of thought which economists now apply to economic problems"—in this case the business cycle. If this is indeed true, the present book must be said to fail miserably in its objective, or the author and the editors have not really made up their minds about the audience being aimed at. There is little doubt that most of its contents will sail right over the head of the "ordinary" or "unitiated" reader. On the other hand, the materials of interest to the specialist are hidden under a bushel; the specialist is likely to be unwilling to wade through all the familiar materials to pick out the few points worth his attention. At best,

[2] For a recent succinct statement on the extent of this ignorance, see R. A. Gordon, "Research on Economic Stability" Soc. Sci. Research Council, *Items* 13, Dec. 1959, pp. 37-38.

a few chapters are probably useful on a reading list for graduate students.

The book opens with two chapters devoted to a good, but not simple, discussion of the generation of cycles through the interaction of the multiplier and acceleration principles—the latter in its "pure" and "flexible" forms. The roles of lags, constraints, and erratic shocks are discussed. These chapters are followed by three more dealing with other influences upon investment. Then follow chapters on the consumption function, money and finance, ceilings as an explanation of the upper turning point, the lower turning point, international aspects of the business cycle, and a (poor) chapter on periodicity and major and minor cycles. The penultimate chapter, perhaps the best in the book, is a very good discussion of the problem of long-run trend and the business cycle. The last chapter deals with problems of controlling the cycle, but the reader will get very little idea of the tools of compensatory fiscal policy and none of monetary policy.

The inadequacy of the final chapter is characteristic of a major failing of the book, viz., a questionable distribution of emphasis. All too often major points and conclusions are given short shrift, whereas minor topics and refinements are analyzed extensively. A related drawback is the author's frequent eagerness to cover all possibilities, e.g., all possible influences on investment. As a result, the reader is apt to lose sight of the forest for the trees and thus be unable to separate the important from the unimportant.

Readers of this review will be surprised to learn, *inter alia*, that the profit rate will not necessarily fall in the face of a declining marginal product of capital, because the profit rate (P/K) equals profit per unit of output (P/Y) times the average product of capital (Y/K), and a rise in P/Y may keep P/K from falling (p. 153). This not uncommon fallacy may immediately be exposed by recalling that $P/Y = MP_k/AP_k$, where MP_k is the marginal product of capital and $AP_k = Y/K$. Then:

$$\frac{P}{K} = \frac{MP_k}{AP_k} \cdot AP_k;$$

Since the AP_k's cancel out, it follows that the behavior of the profit rate depends only on the behavior of MP_k, and the behavior of both is independent of both capital's relative income share (P/Y) and the average product of capital.[1]

I should like to close this review on a more constructive note. The author attempts to give a theoretical underpinning to his discussion of the flexible accelerator by referring to Marshallian long-run equilibrium theory of the firm. A more direct and explicit approach might be to observe that, in equilibrium and in the absence of technical progress and population growth, capital stock is wanted by the firm essentially for two reasons: (1) to produce more output with the same techniques, and (2) to produce the same output with more capital-intensive techniques by substitution of capital for labor. In the

[1] Incidentally, contrary to the author's belief (p. 242), only in the knife-edge case of a linear and homogeneous production function does a falling marginal product of capital necessarily imply a falling average product of capital or a rising "normal" capital-output ratio.

face of diminishing marginal returns, this second source of demand for capital must be a decreasing function of the rate of interest. Hence we can write a capital-demand function as:

$$K = f(Y, i) \tag{1}$$

Differentiating (1) with respect to time, we get:

$$\frac{dK}{dt} (= \text{Investment}) = g\left(\frac{dY}{dt}, \frac{di}{dt}\right) \tag{2}$$

or as a linear approximation,

$$\frac{dK}{dt} = I = a\frac{dY}{dt} - b\frac{di}{dt} \tag{3}$$

Investment is thus a positive function of income change and a negative function of change in the interest rate. To maintain a given rate of investment, income must keep growing and/or the interest rate must keep falling. The coefficient a in (3), of course, is the accelerator, $\Delta K/\Delta Y$, which is seen to define the increase in capital stock needed to produce a given increase in output at a given rate of interest and in a given state of the arts. The acceleration principle as a theory of induced investment is thus placed on simple and perfectly sound theoretical grounds. Any modifications needed to allow for excess capacity, finance problems, etc., can be and have been introduced in the form of a flexible or variable (nonlinear) acceleration coefficient. But the point is that any theory of induced investment must begin with equations like (1) and (2), with modifications introduced *therein*, because these equations provide the only sound "micro" basis for such a theory.

D. Hamberg

University of Maryland

Philosophie des conjonctures économiques. By Léon-H. Dupriez. Louvain: E. Nauwelaerts, 1959. Pp. xiv, 506. 425 Bfr.

This book is a companion volume to Professor Dupriez's *Movements économiques généraux* (2 vol., Louvain 1947), and is based on the latter's empirical and statistical findings. While the *Movements* dealt with the phenomenological aspect of economic trends the *Philosophie*'s emphasis is on the conceptual aspect of these tendencies. In addition to its three main parts (the principles, long-term, and short-term conjunctures) the book includes a helpful glossary of technical terms, and a bibliography of related empirical studies made under the auspices of L'Institut de Recherches Économiques et Sociales of the University of Louvain.

The objective of the *Philosophie* is to replace specific techniques of the "cycle" analysis in favor of a general theory of economic conjunctures. The mechanisms and techniques of modern cycle theories are, in the author's view, more preoccupied with building mathematically coherent schemes than with the explanation of facts—more concerned with a rigorous logic than with a rigorous epistemology. By setting his book philosophically and logically apart from the main body of such theories, the author seeks to formulate a theory of unfolding economic "reality."

The central doctrine of the *Philosophie* is an "economic monism" which rules out conceptual and methodological dichotomies between statics and dynamics, short and long periods, real and monetary analyses, and even economic and noneconomic variables. The theory of economic conjunctures is a theory of the combination of *all* circumstances and movements that determine an economic order.

The fundamental postulate of this theory is that all economic movements emanate from specific human actions but tend toward certain equilibrium. Human actions being conscious and end-oriented, the theory of economic conjunctures is unavoidably teleological: it rejects Pareto's positivism and Keynes's "propensities."

The analytical frame of reference is found in Marshall's marginalist microeconomics (because it rejects mechanistic causation) and in Walras' theory of mutual interdependence (because of its integrative nature). But marginal analysis is considered "intemporal," and mutual interdependence too unequivocal regarding the rationalism of the adjustment process. The author's own theory is a theory of economic *successions* which allows not only for adjustment by small steps through time, but also for nonrationality of human behavior, political exigencies, and social aberrations.

The theory is based on three assumptions: (1) Economically motivated human actions, if left alone, tend eventually to establish a set of coherent economic relationships corresponding to the initial goals. But (2) in each successive step toward these goals, new situations and new states of human consciousness will arise to modify these relationships and to prevent their scheduled realization. Nevertheless, (3) the conjunctions of these forces tend to create an "evolving reality" in which economic relationships, though never precisely established, can be plainly recognized.

In spite of the awesome complexity involved in explaining such an all-embracing reality, the theory's analytical structure appears astonishingly uncomplicated. Statics and dynamics seem to be unified by attempting a "marginalist and temporal solution" between Say's timeless law of markets, and Malthus' instantaneous theory of effectual demand. The compromise: the equality of income and expenditure is neither definitional nor instantaneous, but tendential, allowing for appreciable latitude in the short run but only insignificant lapses in the long run.

The fusion of real and monetary analyses is also attempted in a similar fashion. In the approximation of economic equilibria, money is Keynes' "subtle device for linking the present to the future." Money allows individuals to choose not only among alternative goods at a given time, but also among alternative times for a given expenditure. Extending the "ability to choose" over time, money separates the existence of purchasing power from its exercise and thus relieves the equality of aggregate quantities, not only from their intemporal and instantaneous restraints, but also from their dualistic money-real character.

The combination of micro- and macroanalyses are resolved by the author's attempt to incorporate Say's law of markets and Walras' theory of imputation in a single "concrete context." The unifying element is found in the difference between functional and personal income distribution. The accumula-

tion of individual fortunes and the concentration of economic power due to the inequality in the ownership of factors of production tend to disrupt the equilibrium process of each successive time period, thus making the final outcome different from the scheduled norms. That is, the distribution of national product, by ever-modifying individual motivations and actions, affects the direction and magnitude of aggregate supply, demand, and income.

The combination of these real and monetary factors, in turn, determines the direction and intensity of general economic movements (i.e., secular expansion, long-term movements, and short-term fluctuations). Secular expansion reflects changes in real factors, i.e., quantity and quality of resources, organizational improvements, advances in technology. Long-term fluctuations (the Kondratieff), while basically conditioned by fundamental changes in science, politics, and institutions, are more immediately "caused" by monetary factors and political events. But whatever the conditions and causes, long-term movements are rooted in a host of complex phenomena that are not subject to a simple or specific explanation. Short-term movements (the Juglar) have no valid theory of their own; they are simply systematic and recurrent points in the process of approaching long-term equilibrium. They reflect, on the one hand, the conditioning environment under which individuals make their decisions (the *terminus a quo*) and, on the other hand, the goals toward which individual actions are directed (the *terminus ad quem*).

The book's value can be measured only in relation to the magnitude of its task. The theory of economic conjunctures admittedly lacks the precision of specific, fewer-variable models. And, as a "monistic" theory of all economic circumstances, it suffers from the same frailties and superficialities of all such manifold schemes. Yet in the midst of the intricately detailed issues with which economics copes these days, a work of the scope and orientation of the *Philosophie* is refreshingly nostalgic, and somewhat reminiscent of the intellectual interests of a Smith or a Marshall—Dupriez's own heroes.

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The Manchester School of Economics. By WILLIAM D. GRAMPP. Stanford: Stanford University Press; Oxford: Oxford University Press, 1960. Pp. viii, 155. $4.00.

This able book is an enquiry into the intellectual content of the movement for repeal of the corn laws, which brought Britain to substantial freedom of trade in 1846. Grampp set himself a difficult task, for the movement had no over-all system of analysis unique to it, and, indeed, had no real consensus on the question why the corn laws were undesirable. The Manchester School, as Grampp defines it, consisted of five heterogeneous groups, united only by belief in the immediate repeal of the corn laws: first, businessmen who supported free trade from self-interest, hoping it would lower money wages or stimulate exports; second, businessmen who supported repeal on humanitarian grounds, believing it would increase real wages; third, pacifists, who believed that mutual interdependence would further world peace; fourth, the Philosophical Radicals, who expounded the objections of Smith and Ricardo

to protection; and fifth, the nonconformist radicals, intellectual descendants of the Clapham sect.

Much of the objection to the corn laws had a long history, particularly among the Philosophical Radicals, but the political effort at repeal was most vigorous during the existence of the National Anti-Corn Law League, founded in 1839 and disbanded upon victory. The intellectual history of the League and its sympathizers has been strangely neglected; Norman McCord in his recent history of the League[1] explicitly denied interest in the merits of free trade, and Mark Blaug in his treatise on Ricardian economics dealt with the Manchester School only incidentally.[2]

As a consequence of the neglect of the philosophy of the anti-corn law movement, the myth has been perpetuated that the Manchester School was a group of extreme believers in *laissez faire*, related to the classical economists perhaps as the followers of Ludwig von Mises to the Chicago School in our own time. With respect to the corn laws, the Manchester School clearly did take a more extreme line than the Ricardians. They advocated total and immediate repeal, whereas Ricardo suggested gradual reduction of the duty to minimize transitional problems. Ricardo defended retention of duty to the extent that domestic grain was taxed, and argued for tax reform as a necessary preliminary to repeal. Senior also proposed gradual repeal, and Torrens believed in reciprocity. Mild as these qualifications may seem, they were little less than dealings with the devil to the Mancunians.

On issues other than repeal, Grampp demonstrates that the Manchester School cannot be identified with particularly extreme devotion to *laissez faire*. Cobden defended granting the Bank of England a virtual monopoly on note issue through Peel's Bank Act of 1844, and favored certain restrictions on railway construction and on international capital movements. The Mancunians were united in a belief that the textile industry should not be subject to unique restrictions on its labor force, but otherwise Grampp is able to muster a variety of defenses of factory legislation on the part of the School.

The Mancunians dealt with their policy issues with a mixture of Ricardian analysis, common sense, and ordinary benevolence. Because of the lack of a conceptual scheme of their own, their thinking is far less interesting than the Ricardians' own works. Ricardian analysis loses no interest when it is wrong; in fact, it is not difficult to find writers who consider it most interesting when wrong. Thus, to some extent, Grampp's task was a thankless one; economists will never turn to Cobden and Bright for stimulation and wisdom, as they have turned for so long to Smith, Ricardo, and the lesser classicists. The fact remains that the repeal of the corn laws was one of the great victories for the sort of policies with which economists have traditionally been associated, and with which, let us hope, our profession will continue to be identified. Consequently, economists have reason to acquaint themselves with the intellectual history of this notable event, and Grampp provides them with a convenient and concise means of doing so.

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[1] *The Anti-Corn Law League 1838-1846*, London 1958.

[2] *Ricardian Economics*, New Haven 1958.

Schools and Streams of Economic Thought. By EDMUND WHITTAKER. Chicago: Rand McNally and Co., 1960. Pp. xvi, 416. $6.50.

Here we have another workmanlike text for the history-of-economics course. The author, Professor Whittaker, made a name for himself some twenty years ago when he wrote a pioneering *History of Economic Ideas,* which presented the subject by tracing the history of various doctrines or topics rather than by discussing the contribution of one author after the other in chronological order. Since then Whittaker has added to his reputation by publishing scholarly texts on general economics and economic theory.

Unlike the earlier work, the new book is in the main arranged according to conventional lines. Considering the purpose of the book, this approach no doubt is the right one. At times, though, the form of the earlier "synopticon" reasserts itself, as, for example, in the case of John Stuart Mill. Mill's contributions are discussed in parts of three different chapters which deal, respectively, with "Population and the Laws of Returns," "Theories of Value and Distribution," and "The Law of Markets and the Problem of Depression," all referring to "Classical Economics after Smith."

The conspicuous features of the volume are its encyclopedic scope, its high degree of reliability, and its firm anchorage in the author's own first-hand investigations. His interpretations are eminently sound and based on a lifelong familiarity with the subject. Throughout the book, attempts are made to relate subject matter to political and economic history, philosophy, and to the growth of thought in the neighboring disciplines of politics and sociology. The concluding chapters review the most recent developments in economic theory sympathetically and judiciously.

More than has been common in such texts, Whittaker draws attention to the development of quantitative analysis, beginning with John Graunt and Gregory King and later leading to Jevons, Henry Schultz, and the modern econometricians. Here as elsewhere the student is given not only barren facts and data but insight into the continuity of "schools and streams of economic thought." Thus the title of the book articulates what constitutes its unique character. The ever-present awareness of continuity is no doubt the happy result of the author's earlier concern with the development of individual doctrines and ideas. While the outward form of the volume no longer reflects this approach, it nevertheless suffuses much of its internal structure of thought.

This is indeed a worthwhile addition to the recent crop of useful texts for the history of economics. But, alas, as this crop is gathered, more and more colleges are following a policy of retrenchment in providing instruction in the course for which the texts are designed. It is to be hoped that the superb teaching aids which are now available in the form of texts by Bell, Lekachman, and the author of the book under review will attract the superb teacher and deliver the history-of-economics course from the more and more frequently found catalogue entry: "not given this year." At a time when there is a growing and vigorous interest in the history of the natural sciences, when strong efforts are made to humanize these sciences, economics can ill afford to sever its last, tenuous link with the humanities.

HENRY W. SPIEGEL

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Economic History; Economic Development; National Economies

Technischer Fortschritt und Produktivitätssteigerung. By HANS KRIEGHOFF. Berlin: Duncker & Humblot, 1958. Pp. 151. DM 16.60.

The present volume is a doctoral dissertation at the Johann Wolfgang Goethe-Universität in Frankfurt a. M. Part I is devoted to a primarily microeconomic study of the concept of technological progress. Following Hicks' *Value and Capital* the concept is first defined in a marginalist framework. Following Dorfman and Koopmans the concept is then redefined in a linear-programming and activity-analysis framework. Here, technological progress is simply defined as the adoption of a more profitable program. Whether the new program was feasible all the time but not used previously because relative prices did not justify its use, or the new program represents a wider technological horizon, is immaterial. Krieghoff's defense is partly semantic. A more important defense is that one cannot observe the widening of the technological horizon statistically. Statistically one can merely observe the manifestations of such widening in input and output behavior.

In Part II Krieghoff examines technological progress and productivity for the economy as a whole. Productivity is traditionally defined as the ratio between output and input. To clarify the numerator and the denominator of that ratio Krieghoff distinguishes between three categories of economic goods. First, final goods leaving the production process as its net output. Krieghoff calls this category P. Second, intermediate goods produced and used up within the production process. Krieghoff calls it K (*Kapital*) and lets his capital include the not very significant land. Third, original goods not produced but used up in the production process. This category includes labor and is called A (*Arbeit*). Ricardo, of course, would have proceeded differently. To him, labor was reproducible at constant cost and would thus have belonged to category 2. Land would have been very significant and would have constituted category 3. But let us return to Krieghoff. There are now three alternative productivity concepts, he says, i.e., $P/(A + K)$, P/K, and P/A. The first two are dismissed on the grounds that capital (K) is not scarce in any absolute sense; it is reproducible within the system. Krieghoff concentrates, then, on the last productivity concept P/A as the theoretically relevant and the empirically most varying one. He studies labor productivity as the product of capital intensity and capital productivity: $P/A = K/A$ times P/K, and he quotes available time series by Kuznets for the United States and by Krengel for Germany. The familiar distinction between capital-deepening and capital-widening is made and familiar conclusions drawn.

Only on the last 25 pages of his book does Krieghoff get around to practical problems of measurement. Some, but by no means all, of the pitfalls of aggregation are mentioned, and ways to avoid them are described. The closing pages of the book are devoted to a very brief study of labor productivity in West Germany. Using differential-calculus notation the argument can, I think, be reproduced very briefly as follows: Disregarding technological progress one might assume a simple proportionality to exist between output X and input x of the form $X = ax$, where a would be a constant productivity coefficient. But under technological progress a itself will change. Dating all our

variables we would get a dynamic relationship of the form $X(t) = a(t)x(t)$. Take the derivative of this function with respect to time:

$$\frac{dX(t)}{dt} = a(t)\frac{dx(t)}{dt} + x(t)\frac{da(t)}{dt}.$$

The first term indicates that part of the increase in output over time which is due to the increase in input. The last term indicates that part which is due to the increase in the productivity of input. For the German Federal Republic during the period 1950-1956 Krieghoff finds the total increase in industrial output to have been 93 per cent, of which 41 per cent was due to the increase of the labor force, and 52 per cent to the increase in the productivity of labor. The annual increase in productivity was 5.7 per cent, as compared with 3.9 per cent for the United States.

In his dissertation, Krieghoff has demonstrated his familiarity with the most important theoretical and empirical literature in the field, German and non-German alike. To Anglo-Saxon tastes there is too much semantics in the book. But the productivity concept and the concept of technological progress have also been examined from a statistical point of view. This emphasis on operationality is hardly as obvious in Germany as it would be in the United States. If Krieghoff does not contribute anything new, he does at least provide a lucid introduction to the subject, blending theory and measurement well.

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The Economic Development of Communist China 1949-1958. By T. J. HUGHES and D. E. T. LUARD. Issued under the auspices of the Royal Institute of International Affairs. New York: Oxford University Press, 1960. Pp. viii, 223. $3.60.

For those who have been looking for a short, nontechnical, but objective account of Communist China's economic development from 1949 through 1958, the volume under review is by far the best answer. It is a straightforward factual account, with no application of tools of economic analysis. However, facts are well chosen and presented with fairness, and the comments show well-balanced judgment. Both authors formerly worked for the British Foreign Office, and one (Hughes) also had experience with the British Treasury and the Ministry of Economic Warfare.

The book starts out with the background of the Chinese economy, the economic objectives of the Chinese Communist Party, and the period of rehabilitation from 1949 to 1952. The second part deals with the first and second five-year plans, including a succinct account of the development of the planning machinery and technique since early 1950. In their appraisal of the results of the first five-year plan, the authors have found that achievements in the industrial field were uneven, and that "the increase in agricultural production was not only certainly below what it should have been to keep pace with the increase in industrial production; it was probably barely enough to maintain the living standards of the rapidly increasing Chinese population, let alone to provide a surplus for export on the scale required" (p. 57). In 1958

the people's communes were developed, not on ideological grounds, but "in response to immediate practical requirements" (p. 69). On the subject of Soviet aid, the authors observe that by virtue of its significant contribution to the Chinese industrialization effort, the Soviet Union has occupied a commanding position in the Chinese economy, and the Chinese might welcome the prospect of some alternative source of capital to increase their bargaining power, especially over prices, although such an alternative source does not seem likely to be available in the foreseeable future (pp. 78-79).

The third and fourth parts treat the transformation of Communist China's economic framework. The authors maintain that the extensive development of communications is as much for political as for economic reasons, since it serves to merge the various parts of the country into a single integrated and organic unit (p. 109). The methods used and the problems encountered in the process of socializing private businesses and agriculture are traced step by step, with the conclusion that the transformation "was apparently brought about, not mainly by physical coercion or outright expropriation, but by the application of the strongest possible political pressure short of compulsion, coupled with almost unchallengeable financial inducements and penalties" (p. 95). The effect on agricultural production is taken up together with the official agricultural development program. According to the authors, the prodigious growth of population will greatly affect the achievement of the program goals as well as the country's capacity to export, and has already compelled the government to control private consumption since 1953. Another change in the economic framework is the organization of labor into unions which operate as agents of government policy and therefore do not have much to do with the development that "materially the industrial worker is probably in many ways better off than in earlier times" (p. 120).

In the last part it is pointed out that "the prospects for the future development of the Chinese economy depend largely on the successful limitation of the population's growth, on the capacity of the Chinese to acquire and develop modern industrial techniques, and on the success of the Government in holding in check the demands of the consumer while the capital resources of the country are being built up" (p. 207). The concluding note is that the Chinese economy will probably "continue to develop at an impressive speed" (p. 204).

Much of the authors' observations will most probably receive general agreement. The factual presentation, however, is liable to be criticized for certain inaccuracies. Anyone familiar with the Chinese scene will be quick to point out that two of the names of the so-called "four families" are completely miswritten (p. 83). But mistakes of this type are rare and insignificant. More serious are those errors of fact arising from the authors' reliance, for source data, on the *People's Daily* (the official newspaper of the Chinese Communist Party and government) and Peiping's English-language materials. For example, the statement that "according to official figures, the balance of imports and exports has not been in deficit since 1950" (p. 125) is taken as fact, whereas official data do show an annual deficit from 1950 through 1955. Land is said to have been "normally reclaimed, as in Russia, by state farms" (p. 167),

but actually in China the latter were responsible for only a little over one-quarter of the total area reclaimed during the period of the first five-year plan. Evidence is available to affirm that agricultural production is recorded in terms of "biological yield" rather than amount harvested (p. 165n). While the authors are careful to indicate that the 1958 statistics used in the volume were preliminary data, they have not mentioned that virtually all the 1957 data employed were also early (official) estimates and not final figures. Such defects, however, by no means detract from the value of the book as a general balanced account of Communist China's economic development.

CHOH-MING LI

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Economic Fluctuations in England, 1700-1800. By T. S. ASHTON. Oxford: The Clarendon Press, 1959. Pp. viii, 199. 21s. (U. K. only.)

The study of economic fluctuations for any but the most recent periods bears scant resemblance to modern business cycle analysis. Not only is there a paucity of reliable and relevant quantitative data—or of any quantitative data at all—but the structure of economies was so different as to present fundamentally different problems. The term "cycle" is scarcely appropriate for the eighteenth century, but the ups and downs in economic activity were no less pronounced than in more recent times. Professor Ashton has investigated meticulously almost all conceivable sources of these fluctuations: changes in the weather, which accounted not only for seasonal variations but, through their influence on agriculture, for annual variations which propagated themselves throughout the economy; the effects of the frequent wars on finance and commerce; the rise and fall of activity in the building trades and public works; and the influence of financial crises. In a final chapter he brings together all these various movements to establish the over-all general pattern of economic fluctuation. He finds that, as compared with the nineteenth century in particular, the fluctuations were of greater frequency, shorter duration, and had more abrupt turning points; the evidence does not permit a comparison of amplitudes. To account for the pattern he stresses the overwhelming importance of agriculture, the imperfection of markets for commodities, labor and capital, arising chiefly from inadequate facilities for transportation and communication; the inexperience of bankers and entrepreneurs; and—a familiar theme to readers of Ashton's other volumes on the same general period—the evils of the usury laws in preventing the interest rate from performing effectively its "natural" function as a governor of economic activity. (For a judicious critique of the latter argument see H. J. Habakkuk in the *Economic History Review*, April 1956, pp. 434-36.)

Ashton has done well to present his account in terms intelligible to economists, but like most good historians he has been mainly concerned to depict and explain historical phenomena in their own terms. The book, which took form initially as the Ford lectures at Oxford in 1953, may be regarded in some respects as a supplement to the author's *Economic History of England: The 18th Century* (1955). Although it integrates a surprising amount of quantitative data and some elementary economic theory into the analysis, the method

resembles that of the annalist more than that of the econometrician. In keeping with the tenor of his other works, Ashton stresses the continuity of economic change and plays down the "revolution" in industry in the second half of the century. He seeks to reconcile the indubitable growth of aggregate income and capital with the widespread contemporary (as well as more recent) accounts of hardship and privation by pointing out that examples of the latter occurred most frequently in periods of depression. But in so doing he neglects a factor which he has himself insisted upon previously, the rapid growth of population and urbanization. Moreover, he fails to take into account the possible effects of the distribution (or redistribution) of income, overlooking J. S. Mill's observation in the mid-nineteenth century that it was "questionable if all the mechanical inventions yet made have lightened the day's toil of any human being."

On the whole, however, there is little to criticize in this volume. For reasons already alluded to, it will be of little direct value to business-cycle theorists but, one may hope, will be consulted frequently by social and political as well as economic historians of eighteenth century England.

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Deutsche Volkswirtschaftliche Gesamtrechnung und ihre Lenkungsmodelle 1949-1955. By W. G. WAFFENSCHMIDT and a research team. Stuttgart: Gustav Fischer Verlag, 1959. Pp. 200. DM 14.80.

Here we have probably the first econometric macro-model of the German postwar economy. As such this study cannot fail to attract attention. While it suffers from severe shortcomings—an almost exasperating paucity of data and a statistical expertise that is not good enough for the formidable task—the descriptive substratum is bared, and one cannot out of hand preclude the possibility that the findings will by and large be corroborated by future more sophisticated studies.

First, the authors take annual macro-data (as published by the *Vierteljahreshefte zur Wirtschaftsforschung* between 1949 and 1956) and compute straight least-squares estimates of structural parameters in several linear single-equation models: They regress (undeflated) consumption expenditures of households on household income and on a price index for consumer goods; private investment outlays on the social product of the preceding year and on current profits; government expenditures on government revenues; etc.—all in all over a dozen equations.

In each case, regressors are chosen once for all, without scrutiny of alternative regressor combinations, their relative economic plausibility, and their respective residual variances. Apart from point estimates for the regression coefficients, the authors content themselves with standard errors of estimates as criteria for judging the results. They use uniformly seven degrees of freedom (the number of years of observation), rather than subtracting one for each independent parameter estimate, as standard texts counsel. For equations where they use two explanatory variables (i.e., estimate three parameters) the customary procedure would leave them with just four degrees of

freedom and consequently much larger standard errors than stated. Thus, e.g., in the case of the private-investment function, the standard error of estimate is likely to be about 4.5 billion D-Mark, not about 3.4 billion as quoted; and given the order of magnitude of investment (always below 18 billion during the period in question) a 25 per cent error margin, roughly, is hardly an improvement over casual qualitative appraisals. Likewise the authors estimate *five* parameters from the seven observations for their macro-production function, although not without scruples. Suppose the authors' standard-error definitions were appropriate. Then one could "recommend" taking six explanatory variables throughout; the seven points determine the corresponding regression hyperplane so tightly that the authors' statistic would be *identically* zero! Alternatively, even with only one explanatory variable arbitrary close fits could be enforced by polynomials, say, of sufficiently high degree. But in essence these are tricks of numerical, rather than statistical, probability-model-related, analysis.

Second, the authors devise a simultaneous-equation model. They introduce four definitional equations, discard some of the previously used variables altogether, and single out others as predetermined, *viz.*, welfare payments, government investment, consumption by firms, the social product of the preceding year, and the price levels of export, import, and consumer goods. From this 16-equation model with 23 variables the authors obtain the reduced-form equations, whose parameters are estimated (apparently by least squares; annoyingly we are never told; nor is there any hint about alternative estimation procedures). The deviations from reality are too wide to satisfy the authors; the estimate of the social product of 1955 (e.g.) understates it by more than 10 per cent.

Third, the authors discuss many of the findings mainly from a national-accounting, flow-of-funds, and flow-of-commodities point of view. Partly in order to do this, they investigate several markets in terms of *Warenauftrieb* and *Geldauftrieb*. Some of their economic terminology will strike most readers as avoidably bizarre. To begin with, it is not quite clear to what extent the *Auftrieb* notion ties in with Patinkin's demand (the conventional, Marshallian, plus Wicksteed's reservation demand). But apart from that, the estimation (called "graphical approximation") of such *Auftriebe* will be found entirely unacceptable by all those who insist that problems of estimation have to be approached by first making specific the probabilistic assumptions that underlie the model, instead of proceeding mechanically, with purely formal criteria. For one thing, Waffenschmidt's method, somewhat too involved to be expounded here, seems to beg the question about the constancy of the relevant slopes. For another, it sidesteps rather than solves the problems well known from Elmer J. Working's classical paper "What Do Statistical 'Demand Curves' Show?" (*Quart. Jour. Econ.*, Feb. 1927, *41*, 212-35.)

This, then, is rather an unusual research report. While from a statistical point of view it is unfortunately impossible to herald it as a landmark of quantitative economics, one must, on the other hand, respect the hardihood it took to embark on such a project—with seven points of observation during a period of rapid change. But even extreme partisans of Waffenschmidt's

methodological preferences will perhaps concede that the whole presentation could have gained in appeal and persuasiveness by (a) resolutely conforming with contemporary statistical usages—e.g., interspersing a word here and there on such matters as the likely effects of presumably highly correlated error terms—or (b) pointing out precisely what has motivated departures from such usages and from well-reasoned strategies of recognized masters in the field; (c) including at least a few references to standard sources and to comparable endeavors by other researchers; (d) more austere editing; (e) better graphs with better legends; (f) an index.

Since the term *Lenkungsmodell* somehow suggests direct policy implications, it should be mentioned in closing that the authors expressly and wisely disclaim any "immediate, practical" applicability of their findings.

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Gezira: A Study of Development in the Sudan. By ARTHUR GAITSKELL. London: Faber and Faber, 1959. Pp. 372. $5.95.

The Gezira plain is a broad area 200 miles long and 80 miles across (about half the size of South Carolina or a little larger than Taiwan) lying between the White and the Blue Nile in the heart of Sudan. At the turn of the century, it was a hard land with few trees to break the monotony of the flat, dusty plain, supporting only a sparse population from the crops that were cultivated during the three months of the year during which rain fell. Fifty years later, it was a tremendously productive area, with an irrigated area of a million acres, about a quarter of which was planted in valuable long-staple cotton with a market value of over $45 million. A greatly increased population now enjoyed a standard of living that would have been thought unthinkable fifty years earlier, at a time when the way of life in this barren area had probably not changed appreciably since Biblical days.

The economic development of this area over a relatively short period provides valuable lessons to a world that is deeply concerned with the problem of raising the living standards of backward peoples. The story and the lessons have been told in a recent book by Arthur Gaitskell, who was intimately concerned with the development of Gezira for over 30 years. This detailed and thoroughgoing study provides the stuff against which many an assumption and theory of development can be tested, and it seems likely that many of those so tested will emerge considerably modified.

The Gezira project was what today would be called a joint venture. There were three partners in the venture: (1) private entrepreneurs who put up the operating capital and managed the cultivation and marketing of the crop; (2) the government of Sudan, administered by the British, which provided most of the fixed investment, largely by floating foreign loans; and (3) the tillers of the soil, who operated as share tenants. The project required a heavy fixed investment in a large storage dam, irrigation canals and transportation facilities. It also required a sweeping land reform which made it possible to introduce efficient use of valuable irrigation water and other large-scale farming techniques. The unique feature of the land reform was that no property was

confiscated. The title to the land was left with its original owners, but they were required to lease it to the government at a reasonable rental for a period of 40 years. The land was divided into regular plots which made for efficient management of the irrigation process, and the plots were rented to the cultivators on a share basis. Landowners were permitted to take up such tenancies as they and the members of their family could cultivate. Inefficient cultivators were subject to dispossession by the management. The tenant rights could not be mortgaged, and it was provided that no debts except those incurred for labor performed could be secured by a legally enforceable lien against the crops.

Proper use of the precious water required a regimen totally new to the Sudanese cultivators. They had to follow a strict schedule of planting, weeding, watering and harvesting. It was the duty of the management, which was a private company, to see that all the tasks were performed and keep the operation going smoothly. This required a substantial force of field men to see that things were done properly. At the same time, the tenant had considerable freedom, and his success depended to a large extent on his own efforts. Unlike cooperative arrangements, the profits were not pooled, and so to a large extent success or failure depended on how well the individual managed his own affairs, rather than on how the entire group performed.

However, the individual tenant did get benefits that large-scale operations can bring. The plowing and spraying were done by machines provided by the company. Research, which was of crucial importance since plant diseases threatened for a time to overwhelm the entire program, was provided by the company. Credit to finance each stage of the farmer's operations was also made available to him on a systematic basis and at low cost.

Although the scheme has proven enormously profitable, Gaitskell emphasizes that the profit motive was not allowed to outweigh the interests of the native population. These were carefully safeguarded, though Gaitskell thinks in retrospect that not enough was done to see that their cultural development kept pace with their economic progress. Materially the lot of the people has been tremendously improved, and the country is economically viable.

It is unlikely that this would have come about without the private capital and personal spirit of enterprise which were responsible for beginning the project and advancing it against great difficulties. In reading Gaitskell's account, one can't but be impressed by the importance of the commercial motivations of the management in the over-all success. On the other hand, participation of the government was of vital importance, for it is doubtful that sufficient private capital could have been raised to carry out the project without granting or selling large tracts of land to foreigners. This would have run counter to the desire to safeguard the rights of the Africans and avoid the growth of serious social problems.

According to Gaitskell, high among the reasons for the success of the Gezira project was the policy of making haste slowly. Valuable experience was acquired by means of pilot projects, which took time but which in the end saved both time and money and perhaps averted complete failure. Gaitskell

concludes that "the establishment of equitable and practical principles of development is more important than the pace." This is worth considering these days when there is such a great temptation to throw huge sums of capital into doubtful projects merely because time is considered to be the element that needs to be most economized.

Gaitskell believes that some things ought to have been done differently; for example, he thinks an equalization scheme to even out the fluctuations in the year-to-year profits of the tenants would have been desirable and that progress might have been steadier if the high profits of the good years had been partly carried over to supplement the returns in the bad years. A few years of exceptionally high profits followed by a fall had unsettling effects on the cultivators. He does not believe that it would be feasible to meet this problem either by trying to stabilize the price of cotton on the world market or by diversification of production in Gezira. The former would be unworkable and the latter would run counter to the strong desire of the cultivators to maximize their earnings. No crop that could be grown in Gezira approaches cotton in profitability, and the possible safety that diversification might bring has little appeal when weighed against the loss of income that it would involve year in and year out.

The experiences gained in the development of the Gezira plain could be studied with profit in many countries of the world. One is tempted to agree with a Pakistani observer, Sayed Mohammed Afzal, who, hoping that it might prove to be a useful example for Pakistan and India, described the Gezira scheme as "one of those outstanding experiments on socio-economic problems of the current century which . . . deserves to go down in history as a great romance of creative achievements."[1]

REED J. IRVINE

Washington, D. C.

Italy in International Cooperation. By KAREL HOLBIK. Padua: CEDAM, 1959. Pp. xv, 158. L. 2.000.

Mr. Holbik has written a stimulating analysis of Italy's postwar economic policies with particular attention to her international trade. It is an exciting story, which starts with an excellent account of Italy's economic difficulties, such as shortages of almost all minerals except sulphur, mercury and zinc (the supplies of which are great enough to allow exports), the important role of the government in the economy, and the tragic effects of fascist economic policies. Italy makes up for lack of fuel by extensive use of hydroelectric power (30 per cent of all energy consumed) and natural gas (methane). The per capita income, 40 per cent higher than 1938, was only $428 in 1957. Venture capital is scarce and internal funds are expensive. Interest on bank loans averages 10 per cent. These factors combined with cheap labor have retarded the mechanization of industry. Chronic unemployment (10 per cent of the labor force) and excess population are a constant problem. The labor

[1] Quoted in *Gezira* from an article entitled "A Note on the Gezira Scheme, Anglo-Egyptian Sudan," *Indian Cotton Growing Rev.*, April 1949.

force lacks sufficient skilled labor and many workers are "underemployed." Outside capital (especially U.S. investment in oil and natural gas), Export-Import Bank loans, and World Bank loans have helped the economy. Another difficulty is the underdeveloped South with a per capita income of only $175 per year. The *Cassa per il Mezzogiorno*, a special fund for development of this region, set up by the government, has brought some improvement.

Under the fascist regime, big business became monopolistic and has remained so. The Institute for Industrial Reconstruction (IRI), a government holding company started under fascism, still controls large segments of the economy. The fascist regime aggravated the structural weaknesses of the Italian economy, "a masterpiece of economic and financial lunacy" (Gaetano Salvemini, p. 15), by such means as the battle of the wheat and the revival of mercantilism. These policies led to a sharp decline in the standard of living.

Holbik believes, along with many other observers, that Italians have made their psychological and economic recovery from the war more quickly than some other Western European nations—to some extent because Einaudi brought back the policies of free trade and economic liberalism to Italy. Immediately after the second world war, the Italians were faced with severe shortages of coal and a decline in agriculture (wheat production was down from 75.5 million quintals in 1936-39 to 46.7 in 1947). Finance minister Einaudi carried out a most effective currency reform in 1947. High production costs had resulted from monopolistic practices, made worse by high labor costs, such as the law "blocking" workers in certain industries. Social security costs have increased threefold over 1938. Fluctuating exchange rates were adopted in November 1947. Coupled with effective internal controls this measure gave the Italians the chance to follow an "independent" monetary policy. As time went on, Italy increased its imports of foodstuffs and raw materials and decreased its imports of manufactured products, while exports of manufactured products were greatly increased. In 1946 and 1947, trade with the United States was a major factor (63.2 and 56.7 per cent of imports). This was a temporary expedient, since Italy's best export markets were still in Europe.

The Marshall Plan helped tremendously. As a member of O.E.E.C., the Italian government had to free import trade from quotas. Even before the European Payments Union was instituted, trade was facilitated by intra-European payments agreements. After 1948, Italy ceased to depend on the Western Hemisphere and turned to European countries which were recovering rapidly. West Germany resumed her traditionally prominent place in Italian trade in 1949.

The real solution lay in intra-European cooperation on a liberal basis. Imports poured into Italy because of the European Recovery Program. She used her industrial capacity more completely and net investment went up by 25 per cent from 1950 to 1951. Manufacturing production rose from 107 to 135 (1938 = 100). Along with these improvements tariffs, duties and quotas were reduced. Exports to the European Payments Union area increased and trade with the dollar area declined.

A new tariff law was enacted in 1949 to protect high-cost Italian industry. The average rate was 24 per cent. This was reduced by 10 per cent in 1951, to encourage imports and to reduce her credit balance with the European Payments Union. The Italians reduced their tariffs at the General Agreement on Tariff and Trade conference by about 28.5 per cent, covering about 50 per cent of all imports. At the Torquay conference, Italy made important bilateral agreements with 14 nations, including Germany, Austria and the United States. Imports in 1950 were higher than 1938 by 150 per cent; exports up by 120 per cent. Tariff reduction actually had little effect on the growth of exports, since the difficulties of Italian exporters were due not so much to foreign competition as to the high prices of their products. Import liberalization brought a rapid expansion which continued up to 1956.

The Schuman Plan has aided the Italian economy tremendously. Crude steel production in 1956 exceeded that of 1937 by 180 per cent. The basis of the plan was a more rational division of labor, removal of traditional discriminating practices and a decline in economic nationalism. The changes included greater use of natural gas; financial help from the community to modernize the Sardinian coal mines, toleration of monopolistic practices by Finsider, etc. Italy's dependence on U.S. coal has ended. Twenty-five hundred workers were dismissed in Sardinia, but were retrained or hired to work in foreign mines. Marginal concerns were forced out by technological improvements. This led to the discharge of 8,000 workers in 1953. The Plan helped finance their readaptation with a loan of 3 billion lire.

The lira has maintained an exceptionally stable value when compared with other currencies. The EPU helped in this stabilization program through the multilateral foreign exchange arbitrage, set up among several European nations in 1953, which Italy joined in August 1955. Even under this system Italy's "invisible trade" in convertible currencies provides essential balancing elements.

Holbik believes Italy has attained considerable internal economic growth and not only has expanded her foreign trade but has also recovered an important position in the Western European economy. Since Italy has a rather rigid system of internal prices, Holbik says she has attained a "pseudo-liberalism" in foreign trade.

As a postscript to Holbik's comments, Italy finds itself with a larger gold and foreign-exchange reserve than the United Kingdom (Spring 1960). Production is up 10 per cent over 1958, while exports are up 12 per cent. Exports to the United States are up sharply (40 per cent over 1958), as well as to the Common Market nations. Unemployment is a little lower, but still substantial in the South.

This volume adds a notable contribution to postwar economic history and analysis.

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Flushing, New York

Industrial Complex Analysis and Regional Development: A Case Study of Refinery-Petrochemical-Synthetic Fiber Complexes and Puerto Rico. By WALTER ISARD, EUGENE W. SCHOOLER, and THOMAS VIETORISZ. New York: John Wiley and Technology Press, Massachusetts Institute of Technology, 1959. Pp. xvii, 294.

Given an island rich in cheap labor but poor in most other resources, how is the regional analyst to choose among the many plausible industrial complexes which could be developed on the island for a given level of investment? Such, very broadly, is this book's ultimate problem. The island in question is Puerto Rico. Observing that Puerto Rico's small over-water distance from abundant Venezuelan oil is itself an important additional resource for the island, the authors narrow the problem to a comparison of a number of alternative complexes based on petroleum. Since still another resource is the island's duty-free access to U.S. mainland markets, each complex studied is assumed to send its product to the mainland. The proposed Puerto Rican complexes are to be compared, moreover, with respect to their advantage (in net revenue) over similar projects efficiently located on the mainland. The finding of several complexes for which Puerto Rico has a significant locational advantage would be an important step towards the final choice of good complexes.

Each of the complexes studied comprises three sets of production activities —oil refining, the production of petrochemicals, and the production of synthetic fibers. Since there are economies in spatially integrating petrochemical and synthetic-fiber facilities with a refinery, a study which amputated these facilities would, so the authors contend, be much less useful.

The comparison of alternative complexes proceeds as follows. An activity matrix describing the technology is compiled. Its rows correspond to primary, intermediate, and final commodities other than labor and equipment. Its columns define the proportions in which these commodities enter, or emerge from, a number of linear and additive processes (activities). They include (1) six different refinery prototypes; (2) a group of processes (including a number of alternative processes yielding identical outputs) which use the refinery gases and several exogenous materials to produce eighteen chemicals, some salable as such and others to be used in making synthetic fibers; (3) processes which use some of the eighteen chemicals to produce nylon, orlon, dacron, and dynel.

The description of the technology is next enlarged to include labor and capital. At "unit" level a given process requires a certain number of dollars of equipment and of man-hours per year—either man-hours of skilled labor (assumed to be needed in chemical and refining activities and to be imported into Puerto Rico as needed) or else man-hours of Puerto-Rico-type labor (assumed to suffice in synthetic-fiber production). At the level λ the given process requires these quantities of capital and labor multiplied, respectively, by λ^{α} and λ^{β}, where α and β lie between zero and one and are constants unique to the given process. For each process such smoothly decreasing labor and capital costs are assumed to hold for those values of λ which define the several complexes to be compared.

The next step in the procedure is to choose from the infinitely many possible complexes (each defined by a set of process levels lying in the decreasing-labor-and-capital-cost ranges) an interesting small collection to be studied. Several constraints are imposed on the choice: (a) Each process level in the complexes chosen is to be above a minimum level below which the process is not "economically feasible." (b) The refinery process in all the complexes chosen is to be the same one out of the matrix's six possible prototypes. (c) The refinery's crude capacity is to be the same in all complexes, as is the total synthetic fiber output; these capacities are to be "moderate" ones, common in mainland complexes. (d) Each complex is to contain only one of the alternative processes yielding a given output.

The 28 complexes chosen for study meet these constraints but differ with respect to the scale of a number of the chemical processes and the relative output of the four synthetic fibers. They are described in detail. With the aid of a number of assumptions, for which careful arguments are made, the costs and revenues of each complex are then calculated—for Puerto Rico and for the "best" mainland location (which is convincingly argued to be the Gulf Coast). All 28 programs turn out to have a net advantage for Puerto Rico—the cheapness of local (synthetic-fiber) labor outweighs Puerto Rico's transport disadvantages.

The 28 comparisons are next subjected to "differential-profitability corrections." These patch up the comparisons to allow for the fact that it would not be efficient exactly to duplicate on the mainland a given Puerto Rican complex. For a given Puerto Rican complex, the authors estimate the production costs of a mainland complex yielding the same output quantities but using, for this purpose, the petrochemical processes which are judged to be "best" on the mainland, operated at the scales which achieve "minimum," "moderate" or "maximum" economies. The three scale assumptions yield three alternative amounts by which the previously calculated advantages of the given Puerto Rican complex are to be reduced. The effect of the corrections is drastic. Under the maximum-scale assumption none of the complexes has a Puerto-Rican advantage and under the other scale assumptions only two or three. The study concludes by considering some "reduced" programs (in which important intermediate materials are imported into Puerto Rico). Some of these do appear to have a Puerto Rican advantage.

The drastic patching-up procedure raises the question why not even an exploratory attempt at linear programming was made in the study. Curiously, the authors briefly dismiss the possibility, blaming not only the "insufficiently developed" state of the art but asserting as well that "pressing" resource limitations are the major justification for a linear programming approach and that these are unimportant for industries which might have a Puerto Rican advantage. The best reason for not considering linear programming is surely not this puzzling claim but is rather the difficulty of incorporating decreasing costs (labor and capital) into a linear programming model. (It is only recently that integer programming has offered hope of doing so—see G. B. Dantzig, "On the Significance of Solving Linear Programming Problems with Some Integer Variables," *Econometrica*, January 1960.) If it were not for this difficulty, a

linear-programming model could yield (possibly at great computational cost) all the answers the authors want and more. It could yield, for example, the mainland and Puerto Rican complexes which maximize net revenue for a fixed investment cost. No numbers other than those the authors have painstakingly gathered (the activity matrix, the input, output and transport prices) would be needed; the constraints (a)-(d) and the cumbersome patching-up procedure would be avoided. The model would automatically work out the complicated dependence of optimal process choice on location. Perhaps these advantages might have made an exploratory relaxation of the decreasing-cost assumption worthwhile.

The study is, in any case, a useful and ingenious product of the tools now at hand. Its limitations make one wish the more fervently for the day when less restricted tools are cheaply available.

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Économies des démocraties populaires—les régions économiques en U.R.S.S. Paris: Institut de Science Économique Appliquée, 1959. Pp. 111. 1,000 fr. [pre-reform rate].

Economic Regions in the USSR, No. 5 in a series of studies on the Economies of the Peoples' Democracies, is a translation into French of four articles from recent Soviet Russian periodicals, and a brief comment on one of them, the whole being introduced by a short well-footnoted survey of their significance by Henri Chambre of the Sorbonne.

While each of the articles is an independent entity, the first three illustrate the development of responsible Soviet thinking preceding the decree of May 1957 which decentralized economic organization in the USSR to some extent in the interests of local flexibility and more efficient national growth, it was hoped. The articles are as follows: "On Economic Regionalization and on the Complex Development of the Economic Regions of the USSR," by V. Kostennikov, in *Kommunist,* 1955, No. 14; "The Distribution of Productive Forces in the USSR," by J. Feygin, also in *Kommunist,* but dated September 1956; and "Division of the USSR into Economic Regions," by P. Alampiev, in *Planovoye Khozyaistvo* (The Planned Economy), 1956, No. 6. The two-page critique of the last article is from the same periodical, No. 2, 1957, by A. Danilov, and favors maintaining the economic integrity and individuality of the various republics instead of grouping some and dividing others for the purpose of economic planning.

The criticism by the first three authors of the system of regions then existing in the USSR changes, over the two years, from the rather tentative to the quite specific—but the writing is neither virulent nor dogmatic, as so often in the daily Soviet press. Contemporary failures in production and distribution are attributed, pragmatically, to out-of-date regional concepts, but the suggestions to future planners tend to be innocuous, such as a recommendation that planning be around the idea of a "key industry" in each region, and

that "regional material balances" not be disregarded. All the authors see the problems but fail to come to theoretical grips with them. Fundamentally they all agree that no scientific theory has been developed for guiding rational regionalization, and Feygin particularly criticizes the Academy of Sciences for concentrating on descriptive spot studies instead of establishing sound bases of economic geographical theory as a foundation for regional planning. But none of the authors proposes anything that would startle Adam Smith. In fact, the discussions labor the points of the relative efficiencies of centralized exploitation of natural resources for national commitments versus the uneconomic costs of excessive transportation—an old problem in all large countries. Uneconomic cross-haulages are a main target of criticism, and stress is laid on the need for regional self-sufficiency in ordinary consumer goods and producer goods for local industry (i.e., scaffolding, carpenters' supplies, small motors), but not autarchy, combined with efficient selection and exploitation of regions on the basis of natural resources and energy sources in the interests of the national plan. Manpower availability is mentioned particularly by Alampiev, who emphasizes the problem of reconciling short-range plans from an existing state of development and long-range plans of desiderata in growth, with allowance for previously unforeseen developments such as the enlarged international network of "friendly socialist countries" and the coming availability of huge hydroelectric grids and atomic energy. He also points to the lack of realism in equating vast unsettled nomadic regions, only potentially wealthy, with heavily populated complex entities in the older parts of the Soviet Union.

All three authors go into geographic detail as to the possible desirable regrouping of existing regions, and the reasons therefor, and since apparently many of their specific views were adopted in the 1957 decree establishing 104 Economic Councils (Sovnarkhoz) for the new regions, they are worth examination by economists interested in this field. Their other major value to the U. S. economist is not in the imprecise and somewhat circular theoretical considerations, but in the not inconsiderable factual data scattered throughout.

Of particular interest here are the specific production data (not plans) cited in the last article, "Specialization and Cooperation in Production in the Economic and Administrative Region of Gorki," by S. Prokhorov, likewise in *Planovoye Khozyaistvo*, 3, 1958. Absolute tonnages and volumes of specific items for the Gorki district (formerly Nizhnii Novgorod), a long-established industrial center, are given by the author, instead of percentages only, so that the statistically inclined specialist may find bases of comparison.

The absence of specifically Marxist doctrine is in line with much recent Soviet writing. But the lack of a suitable accounting theory and procedure, often lamented by Soviet economists, remains, in the absence of a free price system, a main stumbling block to evolving a satisfactory theoretical standard for regionalization.

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Statistical Methods; Econometrics; Social Accounting

The Measurement of National Wealth. Income and Wealth Series 8. Edited by RAYMOND GOLDSMITH and CHRISTOPHER SAUNDERS. Chicago: Quadrangle Books; London: Bowes and Bowes, 1959. Pp. xiv, 389. $7.50; 45 s.

In recent decades a great deal of attention has been given to national income accounts while relatively little emphasis has been placed on the measurement of national wealth. Thus we have many well-developed national income statements but few and rather embryonic national balance sheets. This most recent volume of the Income and Wealth Series presents a large quantity of data on the national wealth of various countries and quite a lot of discussion of the methodology of estimating national capital accounts.

The book presents papers on the national wealth of eleven countries: Argentina, Australia, Canada, Colombia, West Germany, India, Japan, the Netherlands, Norway, South Africa and Yugoslavia. These papers were presented at the 1957 Conference of the International Association for Research in Income and Wealth. The results of these studies are summarized in tables that appear early in the book which also include similar data, gathered from other sources, for Belgium, France, Luxembourg, Sweden, the United Kingdom, the United States and Mexico. The tables include estimates of both reproducible and nonreproducible wealth for government and enterprises (military assets are omitted, as is subsoil wealth in some cases), consumers durables and foreign assets. Each of these headings is, of course, subdivided. The dates of the estimates summarized in the tables vary from 1950 to 1956.

Introductory essays by the editors and by T. Barna deal in general terms with the concepts, methods, special problems and purposes of estimating national wealth. There is little that is original in these essays but they help the reader in forming an over-all context for the more detailed papers that follow.

An interesting section of the book deals with the possibility of introducing financial accounts into a system of national accounts. The authors of this chapter, P. J. Bjerve and M. Selsjord of the Central Bureau of Statistics of Norway, observe that in most countries financial statistics are plentiful, but have not been designed to fit into a system of national accounts. To help fill this need the authors develop an equation system that would provide a suitable structure of financial accounts. The system is in some respects reminiscent of the flow-of-funds accounts, but is more structured and more clearly related to the usual national income accounts. It is apparent that many difficulties must be overcome before the proposed system could be put into practice. Aside from any data problems, the system makes no provision for the time dimension, and it was not entirely clear to this reviewer how discrepancies in some cases between nominal and market values would be handled in the accounts.

The remainder of the papers deal with problems and results of wealth measurement in particular countries. These are pretty nearly what one would

expect. The researchers experienced difficulty in obtaining data; they split rather evenly between use of the Goldsmith perpetual-inventory method of estimation and other methods; capital coefficients and other similar ratios change too irregularly to be of much help in describing economic growth.

Two of the essays concerned with particular countries should be singled out for special comment. In the paper on Norwegian national wealth, the authors, O. Aukrust and J. Bjerke, having obtained estimates of net domestic product, total fixed real capital and total employment, regress the first of these variables on the other two and time. A linear function in the logarithms of the variables is used. They find that, for the period 1900-55, the regression coefficient is much larger for labor than for capital. Then taking the first derivatives they conclude that, since the same regression coefficients appear in the derivatives, labor is much more important than investment in Norwegian economic growth. This conclusion is not warranted on the basis of the regression they obtain. It is clearly possible for total net capital to grow while net investment is declining and this in fact occurs in the Norwegian data. Hence a much different regression coefficient would have been obtained if investment had replaced total capital as a variable. Thus the revolutionary finding of the Norwegians appears to be based on a misunderstanding.

Alexander Ganz's essay on economic growth in Latin America is noteworthy. Ganz focuses attention on Argentina and Colombia, Argentina being a relatively mature economy while Colombia is just emerging as an industrial nation. Aside from interesting historical parallels and comparisons of factual information, Ganz brings out the effect of government on economic growth. It appears that there are some excellent object lessons on the way in which government can retard economic growth to be gained from detailed study of certain Latin American nations. In Ganz's opinion the failure of government to provide adequate social capital, especially in transportation and energy, has helped create economic stagnation in Argentina in the past decade.

The economic statistician may shudder as he contemplates the likely errors in most of the estimates of national wealth. The combination of numbers racket and guessing game goes literally unchecked in forming such estimates. But the first stone should be cast by whoever believes he could do better; the reviewer, for one, stands empty handed. The fault is with the data not with the statisticians, and something should be done about this.

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Economic Systems; Planning and Reform; Cooperation

Planning for Freedom—the Public Law of American Capitalism. By E. V. ROSTOW. New Haven: Yale University Press, 1959. Pp. x, 437. $6.00.

This book represents a revision of the William W. Cook Lectures which the Dean of the Yale Law School delivered at the University of Michigan in March 1958. Its general theme is the legal control of the economy, with

special emphasis on the economic factors and forces that today seem to call for control, and on the adequacy of the legal institutions through which control is exercised. The author, who takes merited pride in his success at expounding "economic problems to lawyers, and legal problems to economists" (pp. vii-viii), has directed this book towards a general, educated audience rather than towards specialists in either economics or law, so that its interest for readers of this *Review* will lie in the author's expository skill, his synthesis and his perspective, rather than in the portrayal of anything they will find startingly new or unfamiliar. A two-part appendix reproduces various selected statutory provisions (from the Sherman and Clayton Acts, and the Employment Act of 1946 as amended in 1956), and various statistical tables selected from the President's *Economic Report*, 1959.

The three main areas where Rostow finds that public responsibility comes, or should come, into significant contact with the economic world are: (1) the volume of effective demand as influenced by monetary and fiscal policy, (2) the market for goods and services as influenced by the Antitrust Acts, and (3) the labor market as influenced by whatever form public policy toward labor may eventually take.

On legal, economic and institutional grounds the central core about which the author's entire argument turns is the Employment Act of 1946. That is so legally, because that law serves as "a new field of magnetic force" giving the laws mentioned in the immediately preceding paragraph "new dimensions, a new orientation and a new momentum" (p. 368).

The Employment Act is the focus economically, because public assumption of responsibility for economic stability, even as a somewhat vaguely formulated aspiration, underscores (1) the importance of the magnitude of effective demand; for, even though "there is no one 'true' cause of trade cycles" (p. 117), the fact that "people may spend more or less than their current incomes for current production is the crucial mechanism of business fluctuations" (p. 70), and "the most striking characteristic of the saving-investment circuit is that it is never in balance" (p. 88). Public responsibility for stability underscores (2) the importance of production as "the main long-run force working against the pressure of inflation seemingly inherent in sustained periods of full employment" (p. 25); and "the social costs of monopolistic arrangements . . . as the plausible depression arguments for monopoly are stripped away" (p. 25); and the importance of paying attention to "what happens to prices and wages and profits in the rough and tumble of ordinary business life" (p. 211). It also underscores (3) the "danger of creating a new feudal system of employment with gain of security at the cost of efficiency" (p. 256), so that a new look should be taken at restrictions on apprenticeship and at the immobilities arising out of such things as pension plans and seniority rights. Rostow would like to see a really national labor market established, and he would also like to see productivity become the norm of wage policy.

Institutionally the Employment Act is significant as providing the means by which the President, his executive officers and the Congress *may* keep

abreast of the economic state of the nation, provided the Council of Economic Advisers is sufficiently forthright: "In the competitive world of Washington bureaucracy, shy theorists have never prevailed unaided" (p. 15).

In summary, the author wishes to see more competition, less rigidity, more sustained growth and, within the limits of the possible, more acceptance of competitive market processes as determinants of the structure of production and employment. This calls for some degree of planning, but planning which is oriented about indirect rather than direct controls, since the latter, indeed, represent simply an "attempt to mitigate the effects of a failure of planning" (p. 370). It should go without saying that Rostow is more concerned to plan for a viable capitalism than to essay even the mildest venture into socialism, whose procedures, he remarks, are not only erroneous, but irrelevant to the solution of contemporary problems.

The lectures of which this book is an outgrowth must have been rewarding to hear. They are meaty, clear, thoughtful, and filled with terse obiter dicta on, among other matters, education, foreign policy, our farm and transportation problems. The book does not provide the new ideology which some are beginning to demand, but it does provide a mature and balanced, though not unfamiliar, perspective on the working relationships between government and the economy.

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Overcentralization in Economic Administration—A Critical Analysis Based on Experience in Hungarian Light Industry. By JANOS KORNAI. Translated by John Knapp. London: Oxford University Press, 1959. Pp. xv, 236. $5.20.

This is definitely an important book—candid in tone, rich in insights, abundant in valid analyses. The original was written as a dissertation for the title of Candidate of the Hungarian Academy of Science. The stated aim of the work was to analyze "actual practice" in Hungarian light industry with the aim of drawing up a proposal concerning the reduction in the number of compulsory indices for enterprises, in order to give them more freedom of action. The author did however much more than that: he drew both a detailed and an all-embracing picture of the *modus operandi* of a planned economy of the Soviet type.

The work is divided into six parts, examining respectively: (a) the system of instructions in centralized planning; (b) the system of incentives; (c) the interaction of instructions and incentives; (d) relations between enterprises; (e) the consequences of centralization; (f) the attempts to develop local initiative. Kornai's main idea is that industrial policy—understood as a set of ends translated into a given pattern of investment and targets—and the mechanism of the economy—i.e. the machinery for implementing these aims—are deeply intertwined. According to him it would be illusory to expect real changes in economic performance by tinkering solely with the mechanism of

plan implementation while the aims—i.e. the underlying policy—are left unchanged. Kornai hence concludes that as long as industrial policy aims at an overambitious development postulating rapid growth at any price, the consequences will be shortages of materials, overutilization of capacity beyond the optimum point, a perennial sellers' market, and in short a command economy disregarding completely the wants of the consumer. Within such a framework, reliance on instructions rather than on a balanced system of incentives, on overcentralization rather than on decentralization, is unavoidable. While the author believes that planning is superior to the free enterprise system because of the former's assumed ability to eliminate cyclical waste, he is careful to underline that the planning system has built-in tendencies toward waste of a different nature. Kornai shows in detail how waste results from the reliance on centralized instructions often contradictory in nature, the absence of co-ordination between plans, the conflicting interests of planners and managers, and so on.

After drawing a picture which confirms point by point the Western analyses of the Soviet-type economies, Kornai finally suggests the following proposal for improvement. Let us shift, says Kornai, from a perennial situation of underproduction to a perennial "relative overproduction": in other words, let relatively ample inventories be built throughout the economy and let certain capacities be left unused so that the whole economy would be able to move from a sellers' to a buyers' market situation. Industries will then be able, adds Kornai, to exercise pressures on their suppliers and force them to produce goods according to their actual needs. In turn, the distributing agencies could exercise pressures on the consumer-goods industries and force them to produce products corresponding in assortment and quality to the wants of the consumers. The change from a sellers' to a buyers' market would, along with an appropriate overhaul of the system of incentives, make the efforts of a planned economy to eliminate waste effective and efficient.

This is the gist of the book. As a good communist, Kornai does not dwell directly on the fact that Hungary is committed to a given model of industrialization which postulates the continuous growth of military and producers' goods without considering underlying factor endowments. It is adherence to this model which forces the planning authority to seek rapid rates of growth at any price and which prevents it from moving from perennial shortages and overutilization of capacity to "relative overproduction." But Kornai's suggestion does imply the need of a shift in the pattern of investment in order precisely to secure this "relative overproduction" and achieve a surplus of consumers' goods instead of the present shortage.

Be it what may, Kornai's book is an outstanding essay in the field of centralized planning and deserves to be carefully studied by all students interested in these problems.

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

A Program for Monetary Stability. By MILTON FRIEDMAN. New York: Fordham University Press, 1960. Pp. x, 110. $2.75.

This provocative volume consists of a slightly revised and expanded version of four lectures delivered by Professor Friedman in late 1959 at Fordham University, under the auspices of the Moorhouse I. X. Millar Lecture Series. The first chapter analyzes the role of monetary factors in the major inflations and depressions that have occurred in the United States over the past 125 years. This historical review is followed by three chapters of public policy prescriptions (and proscriptions) intended to reform our present-day monetary institutions and policies. The proposals range from such sweeping changes as the complete abolition of discretionary countercyclical monetary policy to more detailed technical points, such as the preferred way to conduct auctions for Treasury securities.

Friedman argues that the economy has been and is now inherently stable, and that it would automatically tend to maintain high employment with a stable price level if only it were not being almost continuously thrown off the track by erratic and unwise monetary policies. Discretionary countercyclical monetary policies are seen as particularly pernicious; Friedman holds that since the Federal Reserve System was established there has been considerably more instability in the stock of money than previously, and (consequently?) more instability in prices and employment. Countercyclical monetary policies should therefore be quarantined so they can no longer impede the tropismatic response of the economy toward high levels of employment with stable prices. Of course, we will still have minor cycles—after all, "these milder fluctuations have been with us for at least two centuries and doubtless will be for a long time to come" (p. 23)—but in the absence of the aggravation caused by discretionary monetary policies they will be brief and self-correcting.

Three separable although related hypotheses are discernible in the book, only one of which is convincing to this reviewer. That one is the argument that instability in the stock of money has aggravated cyclical fluctuations during much of our history. This is an important and incontrovertible thesis. Indeed, it is a significant part of the case *for* discretionary monetary policies.

Less supportable is Friedman's view that monetary factors have been mainly responsible for our major inflations and depressions. He states that "the failure of government to provide a stable monetary framework has been a major if not the major factor accounting for our really severe inflations and depressions" (p. 9). The evidence adduced is a frequently forced and largely unconvincing interpretation of economic history since the 1830's. In the process, the author completely ignores the significance of such factors as the role of innovation and technological change, the relation between investment opportunities and the volume and type of existing capital, and the cumulative and interdependent nature of many other nonmonetary economic relationships. Even the monetary analysis is unsatisfactory. There is only the most cursory ex-

amination of the direction of causation between historical changes in the money supply and changes in economic activity, although it is inferred that causation runs primarily from money to prices and employment. This is not substantiated by the evidence presented, as indeed, in this reviewer's opinion, it can not be.

Even were Friedman's second thesis true, it would still not be sufficient to justify the third part of his argument: that countercyclical monetary policies should be abandoned since they are likely to do more harm than good. To reach this conclusion, he relies on the likelihood of errors in forecasting and in judgment, and on a rather primitive analysis of time lags in monetary policy. But one looks in vain throughout the book for any systematic examination of the channels through which changes in the stock of money influence the flow of spending, or for a discussion of the cyclical behavior of velocity. Equally surprising, Friedman has almost nothing to say about the conduct of monetary policy during the years since the 1951 Treasury-Federal Reserve accord, perhaps the only period in our history when we have had a reasonably knowledgeable and flexible monetary policy devoted primarily to the attainment of domestic economic stability. Friedman may well be correct in his view that the forecasting difficulties are so great and the time lags so perverse as to make stabilizing discretionary policies all but impossible. But the evidence he presents or that is elsewhere available does not justify that conclusion; short-term forecasting is continuously improving and too little is yet known about time lags to warrant more than the most tentative of hypotheses.

Friedman's two major policy proposals are the establishment of the 100 per cent reserve plan and the replacement of discretionary monetary policies with a fixed "rule." The first recommendation would require cash reserves of 100 per cent for all institutions that accept deposits transferable by check or payable on demand, "whether nominally demand or time deposits." These institutions would not be permitted to make loans except to the extent of their ownership capital. It is not clear whether he intends to include, as the definition would imply, mutual savings banks and savings and loan associations as well as commercial banks.

There were undoubtedly good reasons for advocating the 100 per cent reserve plan in the 1930's. Despite all its shortcomings, it promised to avert any repetition of the bank runs and bank failures that had long plagued our financial system. But today virtually every one of the advantages that might have been obtained from 100 per cent reserves has already been achieved by other means. Friedman admits, for example, that open market operations as currently practiced effectively offset the impact on bank reserves of seasonal and other short-term currency drains, and that the Federal Deposit Insurance Corporation has eliminated the likelihood of bank runs. In fact, he sees the FDIC as "the most important structural change in our monetary system in the direction of greater stability since the post-Civil War tax on state bank notes" (p. 21). What, then, are the benefits to be gained from instituting the 100 per cent reserve plan? They turn out to be only two: the Open Market Committee would no longer have to engage in "continuous jiggling of the monetary tools" to offset currency movements; and we could eliminate the FDIC. We

are told that in comparison with the FDIC the 100 per cent reserve system "would achieve its objectives more effectively and with less intervention into private activities" (p. 69). In order to reap these bounties, we would have to completely overhaul the present structure of the money and capital markets and establish a financial system that would contain within it any number of unexplored problems. Prominent among these would be the source of short-term business financing, now largely provided by the commercial banks, and the problem of evasion in the classification of deposits in order to avoid the 100 per cent reserve requirement.

To eliminate the possibility of discretionary countercyclical monetary policy, Friedman proposes that the Federal Reserve be instructed to increase the money supply at a steady and inflexible rate, regardless of economic conditions. The specific "rule" would depend upon the definition of the money supply adopted: 3 per cent per annum if the money supply is defined in the conventional way as demand deposits plus currency, 4 per cent annually if time deposits in commercial banks are added in as well. However, even a fixed annual rate of increase evidently still leaves too much discretion to the monetary authorities. He further specifies that they should not permit seasonal variations in the money supply and should keep the rate of growth steady "week by week and month by month." What we have then is a .057 per cent per week rule (= 3 per cent per annum) or a .076 per cent per week rule (= 4 per cent per annum). This is advocated on the grounds that to accommodate seasonal variations in the demand for money "would involve introducing an essentially arbitrary element into the behavior of the stock of money" (p. 92).

The issue of "rules versus authorities" in monetary policy seems to me to be a false issue, and the formula recommended appears to rest on shifting sands. Obviously, as Walter Morton has put it, "a fixed rule of monetary policy, or for that matter of any policy, is preferable to administrative discretion, provided that a rule can be found appropriate to all circumstances to which it must be applied. If such a rule can be discovered, then the distinction between rules and authorities will soon disappear because any sensible authority would apply the rule whether or not it was enacted into law. The issue is whether such a rule has been discovered" (this *Review*, May 1951, p. 198).

We certainly need better guides for central bank decisions and better performance criteria than we have today. However, the proposed rule falls far short of what is necessary. It rests upon an arbitrary definition of the money supply (as any such definition must be), and is based almost entirely on the extrapolation of broad historical trends into a continuously changing economy. A case in point is the confident projection of a downward secular trend in income velocity, a trend that has been heading in quite the opposite direction since the end of the second world war. Similarly, the cyclical behavior of velocity, and its implications for monetary policy, are barely touched upon. Increases in the velocity of money have shown themselves capable of supporting substantial cyclical expansions in prices and income during recent years, despite relative stability in the money supply. There is likewise no reason to suppose that a constant rate of growth in the money supply would make the

economy immune to substantial and prolonged recessions during which velocity contracts, as it generally has during such periods.

In addition, to adopt such a rule would arbitrarily prejudge future decisions regarding the policy mix between monetary and fiscal policy, and would thereby prejudge collective decisions regarding the allocation of resources for the production of consumer, investment, or government goods and services. Such a rule, for example, would preclude the adoption of a tight fiscal policy combined with an easy monetary policy in order to channel a larger fraction of resources into the production of investment goods. Similarly, there is nothing sacred about the present level of the federal budget, and substantial changes in that level would imply substantial changes in monetary policy, changes that are quite outside the scope of any simple rule.

Friedman cites the superiority of the rule in comparison with the recent actual behavior of the money supply. The 3 per cent rule generally would have expanded the money supply more in postwar recessions than has in fact occurred, and also would have expanded it more in boom periods. If the 3 per cent rule had been adopted early in 1952, at the start of the first full year after the accord, the money supply would have risen from the $124.5 billion that existed at that time to $157.7 billion by the end of 1959, an expansion of $33.2 billion over the eight years. In fact, it was $144.8 billion at the end of 1959, an increase of $20.3 billion or only about three-fifths as much as would have occured under the rule. Although the rule was ostensibly designed to yield a reasonably stable price level, its application would have meant a 60 per cent greater increase in the money supply than actually occurred between 1951 and 1959, a period during which the consumer price index rose from 113 to 126 and the GNP deflator from 98 to 113.

Although this is a book devoted to economic stability, Friedman never mentions fiscal policy, secular inflation, administered prices, or cost-push phenomena. He makes no policy recommendations outside the monetary area. Nevertheless, he alleges that the adoption of his program would be sufficient to yield fairly effective insurance against major cycles and greater stability from minor fluctuations. It was Henry Simons who said, in 1936, that "the problem of booms and depressions is one which must be attacked from both sides, (a) by policies designed to give us a more flexible price structure, and (b) by measures which will minimize the aggravations attributable to the character of the monetary system and the financial structure. The former attack, however, must always be regarded as primary. The problem of industrial fluctuations cannot be solved, and should not be attacked, exclusively by monetary devices" ("Rules Versus Authorities in Monetary Policy," reprinted in the AEA *Readings in Monetary Theory*, pp. 351, 354).

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The Federal Reserve System. Edited by HERBERT V. PROCHNOW. New York: Harper & Brothers, 1960. Pp. xiv, 393. $6.50.

This book is a collection of seventeen essays which discuss the varied aspects of the organization and functioning of the Federal Reserve System. One

might question the publisher's use of the word "definitive" in describing the work; but it will, nevertheless, most certainly be welcomed by those who seek a better understanding of the role of monetary policy in any stabilization program and of the part played by the Federal Reserve System in initiating and carrying out such a policy.

Virtually all the authors may be considered specialists in their respective fields and to have more than an academic interest in the subjects on which they write because of professional connections with the System itself or with large commercial banks. Despite a large number of authors there is surprisingly little duplication of material and virtually no pronounced differences of style to mar the smoothness of the presentation. Each writer has been allowed full freedom of expression, and while there are some conflicts of opinion among them it would seem to this reviewer that this should add to, rather than detract from, the interest of the thoughtful reader. The editor has done his work with skill and discrimination.

The first chapter deals with "The Historical Background of the American Banking System." The attempt to compress the story of American banking from the colonial land banks of the seventeenth century down to the introduction of the Aldrich Bill in 1912 results in an excellent summary of the developments of that period, but one which would probably be meaningful only to a reader already somewhat familiar with that history. This introductory chapter is followed by one on "The Federal Reserve Act" which outlines the organization of the System, and by a third chapter which acquaints the reader with the meaning and significance of the various items found in a Federal Reserve Bank statement.

The portion of the book likely to be of most interest to those concerned with the role of monetary policy is the section comprising Chapters 4 through 10. Chapter 4 deals with the general nature and limitations of monetary policy. The following four chapters respectively describe the various devices through which monetary policy may be made effective: legal reserve requirements, the discount rate and rediscount policy, open market operations, selective credit controls. The ninth chapter discusses the effects of Federal Reserve policy on the commercial banks, and Chapter 10, the effects on nonmonetary financial institutions.

The next three chapters are mainly of a technical nature. Chapter 11 describes the supervisory functions of the System. Chapter 12 gives us a summary, necessarily brief, of the regulations of the Board of Governors, and Chapter 13 discusses the international activities of the Federal Reserve.

The last portion of the book is devoted to an excellent summary of the history of the Federal Reserve System. Chapter 14 deals with the period from 1914–1929. Chapter 15 covers the disturbed period of the 1930's, and Chapter 16 takes us through the 1940's down to the present. These accounts are mainly factual although there are some interpretations or criticisms with which some readers might disagree. On the whole, though, the opinions expressed pretty generally reflect the consensus of students of the period.

The final chapter is a brief but adequate account of foreign central banking and monetary policy. The author points out the differences in practice be-

tween the United States and foreign countries outside the iron curtain, differences due largely to the varying "economic, financial, and political framework in which they operate."

A careful reading of this volume by any reasonably informed person who desires a further understanding of the role of central banking in our complex economic society should be both stimulating and rewarding. He should come away from it with a greater appreciation of the possibilities and limitations of monetary controls, as well as with a number of questions in his mind to which he would like further answers.

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A Study in the Theory of Monetary Equilibrium. By D. J. BOTHA. Leiden: H. E. Stenfert Kroese N.V., 1959. Pp. viii, 192. *f* 17.

The subtitle, "A Comparative Analysis," is appropriately chosen for this work. Essentially, it is a recapitulation and synthesis of selected contributions by writers on monetary theory. The emphasis is on the role of money in an economy of high employment on the verge of, or afflicted with, excess demands and rising prices. Roughly, the analysis divides into two major areas: (1) the development of a general theory of "monetary equilibrium" which is said to have bridged the earlier gap between monetary and price theory, and (2) the comparison of several models of monetary equilibrium and inflationary departures from it.

In the general theoretical vein, after the Walrasian general equilibrium system, Wicksell receives credit as the first to analyze the economic forces underlying a monetary equilibrium and to specify the criteria for equilibrium. Wicksell went beyond most neoclassical writers, who merely recognized the existence of the real balance effect, by applying it in his analysis. Patinkin fully incorporates the effect of real balances on commodity expenditures, which supplies the connecting link between the money and commodity markets and eliminates the indeterminacy of absolute prices. In so doing, he appears to have provided a "final solution" to the dichotomy between the real and monetary spheres. Within this framework, the bulk of the analysis consists of a synthesis of Patinkin's recent treatment, and a rather detailed presentation of Wicksell's equilibrium conditions, their re-interpretation by Myrdal, and the origin of the Swedish *ex ante-ex post* analysis. Reformulations of Wicksell's second condition involve variations on the theme of equality between *ex ante* saving and investment.

The remainder of the discussion outlines a number of "approaches" to the theory of equilibrium and inflation. In a long chapter on the inflationary process, various possible aspects are exhibited in a series of models. Models on the speed of inflation include a multiplier-accelerator process, a price-wage spiral, T. C. Koopman's "discrete-spending," single-period model of inflationary speed, and a variant of this model by Botha in which production and spending over the period are continuous. A disaggregated, sectored model, that stems from Goodwin and employs matrix algebra, permits incorporation of different

market structures and a combined demand and cost-push approach. The final chapter is an extended outline of a Dutch money-flow analysis associated chiefly with J. G. Koopmans and M. W. Holtrop. It requires a distinction between spontaneous and induced creation or destruction of money, and a similar distinction between spontaneous and induced hoarding, some very laborious concepts. Equilibrium requires equality between spontaneous net creation of money and spontaneous net hoarding, a condition shown to be tantamount to the second Wicksellian condition. This discussion explores a body of literature whose original works are, with few exceptions, available only in Dutch. The Dutch central bank makes some use of a flow analysis by sectors, suggested by this literature, but only the theoretical underpinnings are treated here. Induced hoarding the author holds to be an elusive concept, one that could present difficulties in empirical work.

But this doubt is tolerated only in passing, in keeping with the character of the book. Amidst a procession of inflationary models, one finds only occasional, rather perfunctory conjectures about their applicability to meaningful empirical work.

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A Proper Monetary and Banking System for the United States. Edited by JAMES WASHINGTON BELL and WALTER EARL SPAHR. New York: Ronald Press Co., 1960. Pp. viii, 239. $6.00.

This book resulted from a decision of The Executive Committee of the Economists' National Committee on Monetary Policy that "fundamental errors in principle and practice" (p. iii) in monetary and banking policy should be analyzed and a prescription prepared. The analysis and prescription do not vary substantially from what the same group has been saying for more than a quarter of a century. This is entirely appropriate if the only proper monetary standard is gold coin, without qualification as to the level of prices, output, or employment. Although there are nine contributors (Bradford, Spahr, Niehaus, Kemmerer, Wiegand, Palyi, Phelps, Patterson, and Bell), there is no significant diversity in outlook and only minimal repetition. This book is a well-argued, authoritative exposition of the faults of discretionary monetary and banking policy by dedicated proponents of the automatic full-bodied gold-coin standard.

Spahr believes that both quality and quantity of money are likely to be improper if currency is "managed." There is no need for management, for "all goods and services constitute a demand for all other goods and services, and these exchanges can be effected almost entirely and automatically by the use of self-liquidating credit, the remainder by paper and metallic money" (p. 32). "Credit . . . should rest upon productive activity that will automatically liquidate the credit upon maturity" (p. 33). The choice our society has made for substantial discretionary powers in the hands of government is considered not just a mistake, but positively wicked.

The authors of this book deprecate both the possibilities and need for cen-

tral bank policy as a weapon for attaining sustainable economic growth. The known limitations on the effectiveness of central bank policy (e.g., the absence of control over the velocity of credit) are praised as promoting "freedom." Such freedom is considered much more important than effectiveness of policy, because the economy really does not need much help from central bank policy. The major issues, the authors believe, are integrity of the currency and freedom from governmental restraints. Congress should voluntarily restrain its own power in order to develop the Federal Reserve into a check on the legislative authority.

Spahr argues that the Federal Reserve should be independent of the Treasury and should not become a part of any integrated monetary and fiscal authority. Loss of Federal Reserve independence would reduce market power over the Treasury, undermine the Constitution, and ". . . lead to Executive Dictatorship" (p. 46). This reviewer believes that forced coordination of monetary and fiscal policy probably would be a solution too extreme for the seriousness of the problem. But when two parts of government economic policy are frequently at odds this does constitute a problem worthy of deliberate and temperate consideration.

During recent periods of monetary stringency much discussion has centered on the effectiveness of quantitative controls. Kemmerer says that almost two-thirds of all savings accounts and one-seventh of demand deposits are beyond the control of the Federal Reserve. He also shows how savings and loan associations ". . . are to some extent in the banking business which involves an expansion of credit rather than being strictly in the business of receiving and lending savings . . ." (p. 90). In spite of this he fails to recommend placing nonmember savings banks or other financial institutions under Federal Reserve controls. The desire for "freedom" again overwhelms the argument for effectiveness in policy.

In the view of Kemmerer the "Federal Reserve System [is] too much under the influence of the Federal government and not enough under that of the banking and business world" (p. 66). It is therefore recommended that business interests should choose four members of the Board of Governors and the government three. Other recommended changes would end the Federal Advisory Council and the Federal Open Market Committee.

A model bill is included which provides for a gold-coin standard and some reforms which the contributors believe would increase the effectiveness of the gold-coin standard and the commercial loan theory.

This book is recommended particularly to the student who believes that the major objective of monetary and banking policy is full employment of resources. The authors believe no such thing. They are in the minority who believe that the American economy includes a self-adjusting mechanism which can yield sustainable economic growth with minimal governmental interference. In effect, the authors argue that the profession is wrong and that economists should return to the faith of their fathers.

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Public Finance

The British Budgetary System. By HERBERT BRITTAIN. New York: Macmillan; London: George Allen and Unwin Ltd., 1959. Pp. xii, 320. $5.75.

Sir Herbert Brittain's purpose is to give a comprehensive description and interpretation of the British budget in the light of its growing economic importance since the second world war and its increasing capital outlays. He has portrayed in detail the general design of the budget, sources of revenue, elements of expenditure, the exchange equalization account, the national debt, and accounting and auditing procedures. He has refrained intentionally from pursuing an extended theoretical treatment of such topics as national income, incidence and effects of taxation and expenditures, and fiscal policy. Instead he has given an admirable exposition of the budgetary process from the vantage point of his long experience as Second Secretary of Finance in the British Treasury.

Both in concept and operation the British budget plan has had remarkable success and has served as a model for other Commonwealth nations. Since members of the Cabinet have seats in the House of Commons the budget emerges as a document of combined executive authority and legislative sanction. In this respect the procedure differs markedly from that in the United States where the executive and legislative arms of the government have coordinate status. Also, unlike the British budget, the U.S. federal budget is primarily an expenditure plan, revenue measures having been omitted from the functions of the budget.

A unique feature of the British budget is the traditional practice of separating income and capital transactions by drawing a line "across the middle of the Exchequer accounts" and placing operating revenues and expenditures "above the line" and placing receipts and expenditures applied to debt redemption and borrowing "below the line." Capital payments, however, may be charged either above or below the line, depending upon the nature of the outlay. This is a departure from the practice of commercial firms which rigorously separate income and capital accounts. The author regards as "radical" the proposal of J. R. Hicks that the trading or business activities of the government should be accorded different treatment in the budget from those of administrative departments. Hicks suggests that the accounts of the trading departments "ought to be modelled upon the accounts of a bank" and that the Treasury should exercise the sort of control over these departments which a bank exercises over its clients.[1]

Effective control of the budget hinges on two major functions: first, the formation of estimates; and second, efficient supervision within departments and agencies. Responsibility for the annual estimates rests with the Chancellor of the Exchequer, but he leans heavily on officials of the Treasury who scrutinize the items and purposes of expenditures and the terms and conditions on which money may be paid out. The Financial Secretary of the Treasury presents the estimates for civil departments to Parliament, while defense esti-

[1] J. R. Hicks, *The Problem of Budgetary Reform*, Oxford 1948, pp. 16-17.

mates are submitted by the agencies themselves, though Treasury control also applies to these estimates. Responsibility for efficient supervision devolves primarily upon each spending department, the policy of the Treasury being to avoid as far as possible the details of administration.

The author takes an unequivocal position with respect to the incidence of a national debt, holding that the idea of shifting some part of the burden of war from the present to the future is an illusion. While future generations may be poorer because of the waste of war, "there is no shifting of the real burden from one generation to another." The only sense in which shifting may occur is through borrowing from a foreign country, repayment being made later through exports or some other transfer of value, which involves heavier taxation.[2] It is the opinion of the author that the use of annual sinking funds as a method of reducing the national debt is outmoded and "would seem to have gone forever." Future borrowing in substantial measure is likely to be applied to capital development programs of the nationalized industries.

Within the limitations prescribed, Brittain's book fills an important need for an authoritative treatise on the structure, technique and *modus operandi* of the British budget. It contains many insights on the procedures by which decisions are reached that are not to be found elsewhere. It is timely, thorough and carefully written. It should be of particular benefit to students of public finance, taxation and political science, as well as to government officials.

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International Economics

Trade and Economic Structure: Models and Methods. By RICHARD E. CAVES. Cambridge: Harvard University Press, 1960. Pp. viii, 317. $6.00.

In one respect at least the decade of the 'thirties was a godsend for specialists in international economics. Iversen, Viner, and Wu added their impressive synopses of the theoretical literature on the international adjustment process and international price determination to Angell's pioneering study. Until recently, however, economists have not enjoyed the luxury of a major, up-to-date, English-language digest of the manifold and widely scattered theoretical contributions in international economics. A moment's reflection upon the theoretical and methodological advances in all fields of economics since that time suggests the enormity of this hiatus. The profession is deeply indebted to R. E. Caves' prodigious scholarship for filling in a significant portion of this gap. In *Trade and Economic Structure* economists again have available an imaginative, painstaking, and exhaustive survey of the pure theory of international trade. The book is, without reservation, a reference work of the first magnitude.

Caves' study is not a substitute for the earlier volumes. Rather it complements them. Its scope is limited to a consideration of the theoretical aspects of the simultaneous determination of commodity and factor prices, the quanti-

[2] For a contrary view on the shifting of the primary real burden of a national debt, see J. M. Buchanan, *Public Principles of Public Debt*, Homewood, Ill., 1958, Ch. 4.

ties of factors supplied, and the quantities of goods produced, consumed, and traded internationally, in a world of two or more countries. It is, therefore, concerned to show how the concepts of general equilibrium analysis have been used to build and modify the corpus of pure trade theory. Continuing in the purest of theoretical traditions, its analytical approach is that of positive rather than normative economics. It is a tribute to Caves' intellectual self-discipline that these focuses are rarely lost. But even more pervasive is the author's interest in the application and testing of trade theory. This last perspective, from which he does not deviate, has led Caves to view trade theory as consisting ideally of a "body of well-reasoned alternative hypotheses and models rather than as a monolithic formal structure resting on a minimal set of general assumptions."

This conception has put its impress on the organization of the book. The substantive gambit is an explication of the neoclassical and Heckscher-Ohlin models of trade and production. Haberler's theoretical model is subsumed under the latter formulation since, as Caves explains, the opportunity-costs version enters into the general equilibrium at an intermediate stage and yet is able to derive comparative advantage in money-cost rather than real-cost terms. The succeeding chapter on income distribution summarizes the impact of import duties on relative factor prices, faithfully records the development and qualification of the factor-price equalization theorem, and, especially pertinent to economic development policy, reviews the issues of the Australian tariff controversy. In discussing the role of factor supplies, Caves gives a novel interpretation of the effects of factor-supply variation on the transformation function. A short chapter on modifications of the conclusions of international trade theory necessitated by a relaxation of the assumption of international factor immobility is followed by a discerning exposition of the consequences of alternative hypotheses concerning production functions in the classical and Heckscher-Ohlin models. Here too the general equilibrium effects of technological change, nonconstant returns, and monopoly are examined. The final chapter within the general equilibrium format considers the technical difficulties trade theory faces in incorporating demand influences, analyzes the consequences of shifts in taste patterns, and briefly ponders the absence of any rigorous treatment of demand as an endogenous variable.

Caves' final three chapters develop peripheral subject matter. In the preceding sections welfare considerations occasionally intrude. These scattered comments are systematized by discussing the results of imperfections in the preconditions for a Paretoan optimum and by summarizing the welfare criteria for judging policy alternatives. To partially compensate for the predominance of comparative statics analysis in the major portion of his study, Caves scrutinizes the dynamic adjustments entailed in the processes of economic growth, capital accumulation, international transfer of factors, and technological innovation. The final chapter recounts the results of efforts to test empirically the classical and Heckscher-Ohlin formulations, the theoretical issues raised by Leontief's paradoxical study receiving particular attention.

From all this one vivid impression emerges. While classical trade theory may still retain an advantage in terms of statistical verification, *Trade and Eco-*

nomic Structure testifies to the enormous theoretical fecundity of the Heckscher-Ohlin model. To gain maximum appreciation of this fact the reader must be thoroughly versed in the nature of the questions that pure trade theory seeks to answer. That Caves does not adequately provide this background is the volume's only appreciable defect. Yet, in view of the book's immeasurable contribution to the understanding of the current state of pure trade theory, it is an insignificant blemish.

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Obchodni operace v československem zahraničnim obchodu. By JAROSLAV NYKRYN. (Commercial Operations in Czechoslovak Foreign Trade.) Prague: Orbis, 1958. Pp. 915.

This book, along with three others[1] authored by Professor Nykryn of the School of Economics in Prague, should be unusually useful for students of the Eastern foreign trade monopolies. There is much in the present volume that will interest not only the economist and businessman, but also the lawyer. Although Nykryn deals primarily with the theory and administration of Czechoslovak international trade, this is not a limitation for one-third of Czechoslovakia's national product depends on external commerce—which is a larger proportion than that of any other Soviet bloc economy.

This review concentrates on only two aspects of Nykryn's work: the communist foreign-trade theory (gains from trade); and the planning process which Nykryn describes in unprecedented detail.[2] The parts omitted from this review contain (1) a comprehensive survey of the organizational hierarchy and the decision-making process in Czech foreign trade; (2) the contractual bases of the country's external economic relations; and (3) a description of the legal settlements of commercial disputes between Czechoslovakia and both the socialist and capitalist countries. The emphasis placed throughout Nykryn's volume on contrasts between the Soviet and Western trade systems answers many Western "why's" and sheds some welcome "Eastern" light on the problems of economic relations between planned and market economies.

The communists assert with noticeable satisfaction that one of the distinguishing characteristics of their foreign trade is the mutual assistance promoted by continuous efforts in planning and coordinating their commercial and financial policies. This cooperation allegedly leads to a higher type of international division of labor and thus facilitates progressive expansion of socialist trade. The reciprocal benefit hinges, for instance, on *direct* trade relations (the exclusion of intermediaries), the certainty of planned shipments, and the absence of exploitation.

[1] *Ceny a jine problemy zahraničniho obchodu* (Prices and other problems of foreign trade), Prague: Statni pedag. nakl., 1953; *Mezinarodni platebni styk* (International payments), Prague: Statni pedag. nakl., 1954; *Organisation und Technik des Aussenhandels*, Berlin: Verlag die Wirtschaft, 1956.

[2] Generally, his account of Eastern foreign trading is superior to that found in the well-regarded Soviet publications: S. V. Serebrjakov, *Organizatsia i technika sovetskoj torgovli* (Moscow, 1949) and *Organizatsia i technika vneshnej torgovli* (Moscow, 1958).

Government monopoly imparts additional advantages. It protects the domestic economy against unfavorable external influences (such as cyclical fluctuations) and creates contractual conditions for trade free of the "elements of negotiation" found in capitalist countries which permit strong trade partners to take advantage of weaker ones. A communist government, with vast information at its disposal, is in a unique position to survey, compare, and evaluate sources of supply and sale outlets (which capitalist governments cannot do). In addition, the Soviet-type governments have the financial means to cope with changes in both demand and supply of international goods. These governments enjoy the traditional benefits of bigness, can resort to arbitrary measures, and are capable of giving preference to a political gain even at the cost of economic loss.

East European Communists contend that the classical liberal explanation of foreign trade is based on the capitalist system of production which disregards (Marxian) historical considerations and is inherently exploitative. The liberal theory is incompatible with socialist thinking and practice which rely on planning.

Communist foreign trade theory justifies the existence and desirability of an international exchange of goods and services in four main ways: (1) by internal saving of the labor time needed to produce imported goods; (2) by increased productivity which may result from international division of labor; (3) by reduction of labor input whenever the factor of production labor is replaced by capital (machinery and other labor-saving devices); and (4) by the downward pressure which foreign trade tends to exert on domestic costs of production.

Accordingly, the Soviet bloc countries find that composition of commodity imports and exports is most advantageous which enables them to achieve the maximum saving of social work (effort). But foreign trade has more than an *economic* effectiveness estimated from the point of view of productive factors; it has a *social* effectiveness measured by its influence on national defense, intrabloc cooperation, international political relations, etc.

Productivity (in communist terminology "rentability") of commercial operations is both a clue to, and an indicator of, the financial and real economies to which foreign trade gives rise. (Although "rentable" trade may be defined in several ways, the definition which is least likely to cause disagreement refers to it as that trade which does not require government subsidization and can do without special price considerations by industries.)

The determinants of productivity in trade with capitalist countries differ from those applied to trade among communist countries. In the former case, "rentable" trade depends on the commercial conditions of concluded contracts and on distribution costs, both of which find expression in terms of trade (as a price phenomenon). The communists consider the productivity of capitalist commercial operations to be significantly influenced by the quality of goods sold (since higher quality is expected to result in larger sales, and vice versa), by exporters' ability to sell, by importers' ability to buy, and by other qualitative aspects of commercial transactions.

In addition to the commercial terms of contracts and distribution costs,

productivity of socialist operations is a function of the trading partners' "mutual advantage." This is allegedly easy to determine in communist commerce inasmuch as prospective customers and suppliers are known when production gets under way. In intrabloc trade, price differences and discrimination presumably play a minimal part. At any rate, there is no need for them. Socialist prices are not influenced by factors which loom large in capitalist pricing, such as the time of shipment, the type of foreign exchange used in payment settlements, etc. Furthermore, direct exports and imports minimize charges for banking, transportation, insurance, and other services. Because promotion efforts and expenses are relatively unimportant, they cannot cause price differentials. The same applies to market risks, which are absent.

Since in communist economies the economic effects of *individual* foreign trade transactions are considered inconclusive, the principal theoretical criterion for them rests on whether they are "optimal." This depends on whether international transactions ensure, or guarantee, that the national foreign trade plan will be fulfilled most economically—taking into account all the major objectives of international relations. Clearly, this is an extremely vague criterion.

The optimum is a function of different factors or considerations depending on whether "capitalist" or communist trade is in question. In contrast to commercial relations among free economies, for the Soviet bloc countries prices of exchanged commodities are frequently of secondary importance. As a result, an import transaction, for example, may be optimal as long as the value of the imported good meets the requirement of the existing foreign trade plan. To add to the vagueness of this concept of "optimum," the territorial optimum, which is relevant primarily in trade with capitalist countries and indicates areas with which the most beneficial trade takes place, depends on commercial policy. Little imagination is needed to realize that in this, the bloc countries can proceed in a quite arbitrary manner.

Still another dimension of optimal transactions concluded with the capitalist nations is based on the contribution of export trade to a communist country's foreign exchange reserves. Soviet bloc economists have attempted to measure the increments in foreign exchange supply which result from different amounts of domestic labor engaged in external trade. According to these theorists, the gains from trade are arrived at by taking into account the composite of all foreign operations effected during a definite time period, and are ultimately evaluated in real cost terms.

The long-term economic plans drawn up by the East European countries to achieve "proportional" development of their economies call for corresponding foreign-trade plans. These are complicated because relations with capitalist nations and with the bloc countries follow different patterns. With regard to the former group, communist planners attempt to estimate developmental trends so as to be prepared to change methods of selling, buying, financing, and sales promotion. Because of the co-ordination of the East European

economies, foreign-trade plans have established and preserved a substantial degree of cohesion among them.

Some of the essential considerations underlying a long-term plan for foreign trade are (1) the anticipated composition of imports and exports, (2) the rate of growth of foreign trade turnover, (3) domestic investment as related to foreign trade (and imports in particular), (4) the need for foreign exchange reserves, and (5) availability of, or dependence on, credit.

Vertically, a comprehensive foreign-trade plan is broken down into commercial-financial plans (for imports, exports, and foreign exchange) and into others pertaining to transportation, propaganda, labor and wages, and capital investment. Import and export plans are divided into partial plans for geographical areas. Nykryn discusses in detail the extent to which individual agencies participate in producing long-term, yearly, and quarterly plans, and the statistical-economic analyses used to control fulfillment. Presumably, the Soviet-bloc countries have adopted a system of uniform analytical principles for foreign trade to ensure comparability of planned achievements.

The planners of communist foreign trade believe that the success with which their programs may be put into effect is influenced in considerable measure by the following features of the plans:

1. The foreign-trade plans, like other plans, acquire a binding force similar to that of a law. This plus the responsibilities imposed on those who carry out the plans presumably has had a mobilizing impact on plan execution.

2. The plans are broken down into a large number of tasks and performances whose maximum fulfillment is assured by premiums and prizes awarded to those who make special work efforts.

3. It is possible for long-term plans to be continuously improved on the basis of experience gained in the execution of short-term plans.

4. The planned symbiosis between the economy as a whole and its international sector accounts for the fact that foreign-trade benefits from particularly favorable conditions prevailing in the internal economy. The opposite is, of course, also possible.

5. Foreign-trade plans are set up with regard for a number of economic indicators which are general and therefore reliable enough to ensure optimal results in any phase of international commerce.

6. All activities connected with the completion of foreign-trade programs are subject to reviews and controls instituted not only by the various organs of the ministry of foreign trade, but also by other organizations having some responsibility for, and interest in, external commerce. All these organizations are in the position of jointly curbing any undesirable spreading of disproportionalities in trade flows.

Most readers concerned with the potentialities and limitations of East-West trade will find in Nykryn's work another proof of the existing, sometimes irreconcilable, differences in the nature, administration and objectives of Soviet-type and free foreign-trade systems. The business criteria and commercial considerations which have been sacrosanct for the West are not usually

applicable in the East. On the other hand, it is not likely that the free world will ever succeed in neutralizing the doctrinaire nature of communist trade.[3]

KAREL HOLBIK
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Economic Development and International Trade. Edited by PAUL D. ZOOK. Dallas: Southern Methodist University Press, 1959. Pp. viii, 134. $3.00.

This collection of eight papers was selected from those presented at two conferences held at Southern Methodist University in 1958 and 1959, under the auspices of the Jno. E. Owens Memorial Foundation. The papers deal with the conferences' subjects, "International Trade and Economic Development" and "United States Foreign Economic Policy," but they are not closely related to each other. In the development field, emphasis is given to Latin America. Written by academicians and international and United States officials, the papers are addressed to the general public as well as to a university audience, and they reflect in varying degrees this dual objective.

In the first paper, Howard Ellis forecasts an increasing absolute volume of international trade. He bases his optimism on such factors as population increase, development aspirations of the underdeveloped countries, reduced flows of labor and capital (making commodity trade more profitable), and the transfer of techniques—conditions different, at least in degree, from those when Robertson and Viner made international trade predictions in the 'thirties and 'forties (reprinted in the *AEA Readings*). Contingencies that Ellis says could qualify or even reverse his forecast are a new wave of trade restrictionism or possibly the really economical harnessing of atomic power. Ellis concludes that the United States gains from increased trade stemming from foreign economic development, and he advocates an increased foreign economic-aid program.

Howard Piquet, in his paper, implies that United States aid policy is not deficient, but he deplores the present policy of drift in the trade field. Recognizing that free trade is politically impossible and that protectionism is internationally dangerous, he proposes a more liberal policy based upon government assistance to domestic industries that are forced to adjust to increased imports or a policy of permitting imports to increase on a selective basis, using some variant of the tariff-quota device.

J. Carter Murphy, applying Viner and Meade customs-union theory to the European Common Market, regards favorably its possible impact on the pattern of free-world production and trade. He is also optimistic as to possible economies of scale, and he concludes that the Rome Treaty provides a recipe that economists can endorse.

Wendell Gordon distills several chapters from his recent text on international trade in a paper on international investments. In questioning the contribution that foreign investment makes to economic development, he cites historical evidence that such investment grows by accretion (reinvested earn-

[3] *Cf.* Oscar Lange, "The Economic Laws of Socialist Society in the Light of Joseph Stalin's Last Work," *International Economic Papers,* Vol. IV, London and New York 1954, pp. 145-80.

ings, capital gains, etc.) rather than through the export of real capital goods. He concludes, therefore, that foreign investment, as distinct from technical knowledge, managerial skill, and entrepreneurial initiative, does not contribute much to economic development. He suggests that underdeveloped countries should meet their capital-equipment needs by paying for them with exports, thus avoiding the service and repayment burdens of foreign investment. He does not, however, explain how most underdeveloped countries can generate enough exports to pay for substantial capital imports without first undergoing some economic development and how this initial development can take place without foreign capital imports. Nor does he explain why such capital imports should be discouraged if they more than pay for themselves out of the new wealth that they help to create. Gordon also criticizes the view that average interest- and profit-rate differentials explain foreign investment. He says that such investment is made on the basis of individual projects and is influenced by social, technical, and political considerations. However, to attribute to Ohlin and Iversen average interest- and profit-rate differentials as the explanation for foreign investment is an oversimplification of their views.

Richard La Barge's interesting case study on the economic development of Honduras singles out foreign investment as the initiating cause for the economic growth that has occurred there since 1943. He claims that foreign investment stimulated domestic savings and investment and that it should have the same effect in other capital-poor economies where foreign-owned enterprises have an important role in producing goods and services.

Reynold Carlson and Jorge Del Canto write on how the International Bank for Reconstruction and Development and the International Monetary Fund, respectively, aid Latin American economic development. Carlson emphasizes the Bank's noncapital assistance and Del Canto concentrates on the Fund's efforts in combatting inflation. Sydney C. Reagan discusses United States export programs for surplus agricultural commodities since their inception in 1954 to 1957, briefly analyzing their effects on the recipient countries, on other agricultural exporting countries, and on the United States.

For the most part, this is an interesting, albeit uneven, collection of papers. Like most compendiums of its kind, it would have been much improved if the papers had discussed various aspects of some central topic rather than various topics in the same general field.

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Capitalismo mondiale e cartelli tedeschi tra le due guerre. By J. J. LADOR-LEDERER. Turin: G. Einaudi, 1959. Pp. xviii, 400. L. 2500.

World Capitalism and German Cartels in the Interwar Period ought to have a translation in clear and nontechnical English. In a cheap edition and well advertised the book would reach the wide audience it fully deserves. The book retells the story of the German "miracle" of the years after the first world war. The parallel is West Germany's economic rebirth after 1947. The *leitmotif* is the machinations of international finance and the cartel structures

of the 'twenties and 'thirties; the counterpoint, the men in Brooks Brothers suits now crowding Brussels and Geneva.

The stress that Lador-Lederer gives to the "functional character of international cartels," to their almost "necessary" development, leaves the reader with the certainty that a structure as gigantic and as efficient as that described —a structure, incidentally, that remained intact in spite of the second world war—is more than a fact of history. Its impact on current economic affairs must be recognized and needs continuous study. Consideration of existing cartellization and its effects upon economic interdependence must form part and parcel of any analysis of developments in the European Common Market and consequently of the European economic policy currently followed by the United States. The author's central thesis helps validate recent rumblings characterizing our European policy as self-defeating. This point of view, apparent, *inter alia*, also in the Staff Report of the Joint Economic Committee of Congress, argues that in promoting the Common Market we have helped the development of German hegemony in Western Europe which—through a new Russo-German Alliance—will eventually challenge the leadership of the United States in the Western World.

In relating developments of the interwar period the book shows in detail the interdependence of the requirements of the cartels with those of the European states; the requirements of both expressing themselves at the "supranational" rather than the international level. Similar interdependence of economic interests is at the root of contemporary action advocated and implemented under the banner of integration. When the historical data provided by the book are viewed as building blocks and joined with the mortar which is the work of men such as C. Wright Mills and Burnham—and the book is replete with their ideas—an imposing analytical structure is developed. What follows from it, implicitly rather than explicitly, is the thesis that a truly cosmopolitan power elite is one of the central social facts of the contemporary world.

Beyond its timeliness and intrinsic importance the book deserves notice on three counts: it is a serious work based on a minute and thorough dissection of available facts all of which are carefully documented; it is a clear contribution to the economic history of Europe for the interwar period; and it throws much light on the use of economic power for war. The book's arrangement is essentially chronological. The disorder and the morass of 1918 to 1923; the recovery from 1923 to October 1929; the crises of 1929 to 1933; the transformation to a war economy, 1933 to 1939 are reviewed in turn. A concluding section describes the economic problems of the war and provides a set of "Conclusions on Germany." There is an extended bibliography.

The use made of Germany's international business relationship for war is shown in the economic use of the fifth column, in the frustration of the economic development of her allies and in her capitalizing on foreign industrial expertness. Differences between German and non-German industrialists are viewed in terms of the commitment of the first to war and of the second to the maintenance of business. American business interests are thus painted as having interests in cartels that go beyond battle lines, as having equated op-

position to the New Deal with isolationism and, to the extent that they were influenced by economic consideration linking them to German financial ventures, as having reduced "the potential of the arsenal of democracy."

A work as embracing and as ambitious as this one necessarily has already-known material, has occasional errors and at times overstates its case. The author reflects his own life—as a Marxist economist, the victim of German concentration camps, minister in charge of economic planning for Tito, and now with the same duties in Israel—in an approach critical of capitalism and with a deep attachment to human values. This explains his conclusion that technical solutions of the cartel problem are useless as they must necessarily crumble in the face of "the satanic laughter of economic realities." His pessimism, disturbing though it is, cannot be vouchsafed. The book deserves careful reading.

OSCAR ORNATI

New School for Social Research

Business Finance; Investment and Security Markets; Insurance

Les choix des investissements. By PIERRE B. D. MASSÉ. Paris: Dunod & Co., 1959. Pp. xvi, 489. 4,900 fr.

This remarkable study of the normative theory of investment has no real parallel in the literature in its unique blend of theoretical and practical work, nor are conditions elsewhere than in France likely to make such work possible (except perhaps in the Netherlands and Norway). The author has been Associate Director General of Electricité de France (the nationalized electricity industry) and is currently High Commissioner for Planning, the position created by Jean Monnet. What is extraordinary is that the practical problems of the electricity industry and particularly of its investment program should be governed by a rigorous theoretical analysis which indeed turns out to be the only truly practical way of proceeding. While the application of mathematical methods to industrial production and transportation problems has gone on apace in this country, I believe it correct that very little indeed has been done with fixed investment as far as practical problems are concerned. Even the study of inventory policy has so far been more theoretical than applied.

The important strides in the joining of rigorous analysis to planning problems in France by M. Massé and his younger colleagues (among whom might be mentioned, without any pretense of completeness or ranking, Maurice Allais, Marcel Boiteux, Robert Gibrat, Jacques Lesourne, Edmond Malinvaud, and Georges Morlat) are too little known in this country. To judge by the ignorance of the French language among our graduate students, the situation is unlikely to improve in the near future. Incidentally, it is doubtless significant that nationalization in the United Kingdom has produced no comparable literature.

The present book deliberately avoids any claim to completely systematic treatment. Almost every aspect of the problem of investment choice is touched on and the general principles discussed, but within each topic intensive dis-

cussion is confined to selected topics, usually arising out of the work of Electricité de France or one of the other nationalized energy industries. With the exception of a few simple examples for didactic purposes, the cases discussed involve real decisions of considerable magnitude, a fact which contributes greatly to their fascination.

The first four chapters deal with investment choice under certainty. The general criterion is that of maximizing the sum of discounted profits, where, however, the opportunities for reinvestment must be included in the decision process. The second chapter immediately shows the practical and theoretical character of the work; it deals with criteria for the economic length of life of equipment taking account of both wear and tear and obsolescence. Even under simple hypotheses about technical progress, the problem is difficult.

After a good, though standard, treatment of linear programming in general, the author proceeds to an exposition of the development of the investment plans of the French electricity industry through 1975 by means of linear programming. The various complications are worked out in considerable detail, in such a way as to give the reader an excellent feel for the process of formulating a model for analysis of a real situation.

The next four chapters deal with investment planning under uncertainty and are, to me, the most exciting part of the book. The first of them deals with the criteria for behavior under uncertainty. In addition to an excellent discussion of the various concepts in the literature, such as expected utility, subjective probability (he follows here de Finetti's formulation rather than Savage's) and minimax, he gives an excellent discussion of the conditions under which the law of large numbers does or does not permit one to replace probability distributions by their expected values. The following chapter gives the best discussion I have seen anywhere of the concept of a strategy and its special role in dealing with uncertainty. This is illustrated by a masterly discussion of the proper behavior of an insurance company in limiting its risks. Though a strategy in principle requires considering consequences into the indefinite future, some conditions are given under which there is no loss in considering only a few periods ahead.

The major applications are to inventories, the use of hydroelectric reservoirs with both demands and water inputs random, and production of coal with random demands and import prices (due to P. Gardent). These are handled with admirable clarity and originality. There follow discussions of the rate of exploitation of a mineral resource of unknown magnitude, electric plant under conditions of uncertainty as to water supply and demand, and problems of meeting disaster (e.g., floods) and achieving flexibility by appropriate equipment. It is impossible to summarize easily the rich variety of results.

The last two chapters are more general in nature. There is a summary of modern welfare economics, with particular regard for its implications for the behavior of a nationalized enterprise. For the first time in the book, there is a discussion of criteria appropriate to conditions of decreasing cost. It is interesting indeed that so much planning seems to be perfectly possible in practice

on the basis of the constant returns assumptions of linear programming. It suggests to me that the problem of allocation under increasing returns may not be as difficult in practice as it seems to be in theory.

In the final chapter, the author eloquently expresses his "neoliberal" viewpoint—the competitive system within a governmental framework designed to remove its deficiencies, particularly those arising from increasing returns and the need for coordination of attitudes towards the future.

The reader by now has doubtless inferred that I regard M. Massé's book as well worth study.

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The Management of Corporate Capital. Edited by EZRA SOLOMON. Graduate School of Business, The University of Chicago, Series 3. Glencoe, Illinois: The Free Press, 1959. Pp. 327. $7.50.

For those who have not followed too closely the growing literature on the subject of capital allocation decisions, the title of this work may not appear particularly attractive. One's first instinct would be to view it as a book solely of interest to specialists in the field of business management and finance. Nothing could be farther from the truth. Although it is difficult to conceive what other title the editor could have chosen, the fact is that the rich contents of this work are of essential importance to many different disciplines, including economic theory, accounting, statistics, and managerial economics. Their ultimate significance for business consultants, economists, and executives —indeed, to all concerned with the most rational and efficient disposition of capital resources—cannot be too strongly emphasized.

As an addition to the new series of publications sponsored by the Graduate School of Business at the University of Chicago, this valuable work more than justifies itself as a source of new knowledge and ideas contributing to the improvement of business practices in this vital area of decision-making in the capital investment process. Solomon has done a masterful editorial job. Twenty-two essays have been carefully selected from journal literature of the past seven years and reprinted here in an order that insures continuity of thought, a progression of more involved analyses, and a deepened insight into the more intricate problems surrounding the subject. The work is methodically divided into seven sections: (1) introduction, (2) measuring investment worth, (3) the cost of debt and equity funds, (4) general solutions to optimal investment decisions, (5) special aspects of capital measurement, (6) discount tables, and (7) classified bibliography. Most of these sections contain the finest and most closely reasoned disquisitions that have been offered to deal with the management of corporate capital. The bibliography is similarly well organized and reflects the distinctively comprehensive character of the work.

In his introduction, the editor performs a most useful service by reviewing the common objectives of the articles, the mutually held assumptions of the authors, the areas of agreement among them, the problem areas, and the un-

settled issues. The reader is given a full view of the forest before he ventures into the woods. All the writers are bent upon the formulation of a rational approach to capital-allocation decisions. All are concerned with how this problem should be solved rather than how it is being currently solved by business. Each recognizes that the optimum solution presupposes interrelated decisions on the chosen magnitude of gross investment, the composition of expenditures, and the composition of financing; and each analyst also agrees that present business practices in this area are not based sufficiently on a rational approach. For example, the measurement of profitability on the basis of the pay-out period or the ratio of net conventional income to average book-value of utilized capital is rightly deemed as an inadequate solution. Instead, the discounted-cash-flow method is advocated, which is essentially the process of finding the interest rate that discounts future earnings of a project down to a present value equal to the project cost. Throughout all the articles a persistent attempt is made to develop rigorous, logical concepts for capital budgeting; and every conceivable factor, ranging from opportunity costs to residual values, comes in for incisive analysis.

The outstanding value of this volume is that it lays before the reader the breadth and depth of logical thought on processes and methods for rational capital budgeting. The work is in essence an embodiment of source material for constructive use in the various disciplines mentioned above. Joel Dean's contributions, for example, reveal a systematic endeavor to apply formal economic theory to the general problem of capital allocation; and the articles by Alchian, Solomon, and Durand, not to mention the others, bring into play interest theories, cost-of-capital concepts, and imperfect market considerations. Although the patient reader will be duly impressed by the strides made toward a sound conceptualization of the many problems converging in this area of business operation, he will doubtless form certain balancing reservations supported by the roles of uncertainty, experience, and even business intuition in our permanent environment of economic contingency.

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Studies in Company Finance: A Symposium on the Economic Analysis and Interpretation of British Company Accounts. Edited by BRIAN TEW and R. F. HENDERSON. National Institute of Economic and Social Research Economic and Social Study, Vol. 17. New York and Cambridge, Eng.: Cambridge University Press, 1960. Pp. xix, 301. $6.50.

Heuristic in nature, *Studies in Company Finance* is a pioneering empirical inquiry into the financial characteristics and practices of the bulk of publicly owned British companies engaged in manufacturing and distribution whose securities are listed on a stock exchange in the United Kingdom. While this volume involves a different analytical technique and is more comprehensive than its earlier companion book, *Company Income and Finance,*[1] it may, nonetheless, be regarded as both supplementary and complementary to the latter.

[1] The National Institute of Economic and Social Research, *Company Income and Finance 1949-1953*, London 1956.

In the first of three major sections is presented an analysis of the aggregative information on 2549 continuing firms in the period 1949-1953. Basic data are derived from the published consolidated financial statements required of publicly owned firms by the Companies Act of 1948. The second section consists of detailed analyses of the financial characteristics of particular industries, among them brewing, building, electrical engineering, and retailing; in view of the significant part that industries such as the chemical, shipbuilding, and automotive played in the postwar renascence and structural rearrangement of British industry, it is regrettable that individual treatment was not extended to them. The third section contains the statistical appendices where the aggregative data on firms included in the study are classified according to industry, size, and rate of growth.

Perhaps one of the most interesting features of this study is the classificatory system employed to organize and evaluate the data. Sixteen "indicators" are used to classify the various financial relationships existing in a company and/or industry. Included are: net investment-net income ratio; thrift (ratio of net retained income to net income before interest and dividends); self-financing (ratio of thrift to net investment); liquidity (percentage of net liquid assets to net closing assets in 1953); capital issues of all kinds; and growth in net assets.

While many of these indicators have been commonly used in other studies, the addition of several new ones by the authors, such as thrift, net investment-net income ratio, and self-financing, should prove of interest and value to others. In addition to the classification by "indicators," firms are further classified into 21 industrial groups, 7 size groups, and 4 growth groups. Cross-classification by two or more indicators or groups permits the subdivision of the 2549 companies into 147 subgroups.

Effectively refuted by the findings are certain widely accepted beliefs about the postwar economic behavior of British firms: (1) Although it is ordinarily believed that the majority of investment by British firms was financed through borrowings, this study shows that 83 per cent of their investment in tangible fixed assets and inventories was self-financed from gross savings, i.e., retained income plus depreciation. (2) The burden of taxation was met at the expense of lower dividends, not, as is often supposed, by a lowering of the ratio of retained earnings. For the period 1949-1953 listed companies retained about 25 per cent of their pretax net income compared to 18 per cent in prewar years. On the other hand, in the prewar years dividend payout accounted for 68 per cent of pretax income but only about 33 per cent in the early postwar period. (3) Definitely shown by the authors is the importance of capital issues as a source of external funds for many companies. Capital issues (common and preferred stock, and debt of all kinds except trade credit) accounted for 28 per cent of total investment and 27 per cent of the growth in net assets. The observation is made that reliance on the new-issues market appears much more closely related to the rate of growth of an industry's or firm's net assets than to any one use of funds such as net expenditure on tangible fixed assets. (4) Contrary to common opinion, bank credit in the aggregate did not represent an important source of funds. Approximately 46 per cent of all com-

panies were never in debt to their banks, and bank credit accounted for only 3.8 per cent of the growth in net assets. Larger firms, not the smaller companies, utilized bank credit more often as a means of financing.

The findings on concentration, profits, and growth for this period are not dissimilar from experience in the United States. The study concludes that on the basis of size: (a) profits, on a percentage basis, for the larger companies rose more rapidly than for others; (b) larger companies carried out relatively more investment per pound of income; and (c) larger companies obtained more capital from the open market. Mergers were more important to the expansion of smaller firms than to the larger units. Of particular industries, those not technically stable and those most capital-intensive grew at a faster rate and consequently required large amounts of funds from all sources.

In summary, *Studies in Company Finance* constitutes a substantial contribution to knowledge of the financial relationships of the most important publicly owned British companies in manufacturing and distribution. The analytical system is excellent, and the coverage, both quantitative and qualitative, is thorough. Empirical studies of American firms could be wisely based upon the approach used in this book.

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Untersuchungen über die Investitionsentscheidungen industrieller Unternehmen. By ERICH GUTENBERG. Köln and Opladen: Westdeutscher Verlag, 1959. Pp. 230. DM 23.-.

In this book on decision-making for capital expenditures, Erich Gutenberg restricts his study to a microeconomic point of view. His main interest is to bring into focus the motivating forces behind management's decisions to commit itself to a major capital investment program. It is not a study of cause and effect of investment decisions on the total economy. It is not a quantitative correlation-regression analysis of capital expenditures and determinants. Rather, the author uses the more direct individual interview approach to probe into the concrete investment decisions actually made by major German firms during the three-year period 1954 to 1956.

The inquiry then is not how an enterprise nor an entire industry would propose to act under certain conjectures but rather how they actually did react to economic forces during the test period. The "why" rather than the "how" is the focus. It is precisely by this approach that the author is able to make a contribution to the otherwise profuse and in many cases, incongruous field of inquiry on the general subject of capital investment expenditures. This research into motivations distinguishes Gutenberg's report to a rather marked degree from similar studies by Schindler, Katona, Eisner and Terborgh.

The first two chapters are devoted to the incidence of capital expenditures. Chapter 3 develops the interview approach as used by Gutenberg.

The three main areas of investigation used in the study are discussed in Chapters 4 to 6. They cover the problems of timing and motivation, planning

and evaluation (*rentabilität*). The area of timing and motivation is further sectionalized into replacement and expansion expenditures with the latter furthermore subdivided into investments made either for the purpose of vertical integration, product diversification or added capacity per se. The impact of such factors as competition, cost behaviors, social and legal implications, labor markets, etc., is incorporated in the findings. These are based on comprehensive interviews with 76 national firms in 9 basic industries: coal, oil, steel, chemicals, electronics, machinery, automobiles, textiles and brewing.

In the remaining chapters (7 to 10) the author reviews organizational budget and control aspects as well as "justification" and "cost evaluation" methods used by these plants in conjunction with investment planning. Still, it did not escape the author's attention that the impact of that vast area of imponderables which constantly defies measurement, motivated to a large degree the final decision in many instances; and it is not surprising that he should come to the conclusion that the ability and maximum financial strain on the total financial position of the individual enterprise influenced strongly the final decision. It would appear that the author accomplished his objective by establishing, at least to a degree, the "weighted" impact of each motive on the total capital investment policy.

The work concludes with a summary and comparative analysis of investment motives and appends a detailed case study describing motivating forces behind a decision to construct a new plant for the manufacture of synthetic fabrics.

The author quotes and compares U.S., English and German publications. The work presents a scholarly and well-organized approach to this very controversial area. Since by far the greater part of the report is based on the actual decisions made, it affords an opportunity for comparison with the strategies used by U.S. firms as against those made by similar enterprises on the Continent. Also, the study should be of interest especially at a time when U.S. capital is seeking investment opportunities in the European market.

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Corporate Financial Management. By Raymond P. Kent. Homewood, Ill.: Richard D. Irwin, 1960. Pp. xv, 851. $7.50.

This book is a new addition to the growing list of high-quality texts in the business finance field. As the name suggests the presentation is management-oriented, the emphasis at all times being upon the way in which business executives formulate policy and arrive at decisions. To enable the student to understand this process an adequate description of institutions, arrangements and techniques is presented. However, the author always stresses "why" rather than "what" or "how," since he feels the study is quite incomplete if the student does not clearly comprehend management's reasoning in arriving at a decision.

The organization of the text departs somewhat from the traditional pattern in that techniques of financial analysis and control as well as factors to be

considered in determining the financial structure are discussed before the sources of financing themselves are described in detail. Parts I through IV take up the business corporation, finance functions and cash flows, asset management, and cash budgeting and control in that order. In Part V the issue of choosing the financial structure is considered. The next five parts then cover the major sources of financing which the author divides into the categories of common stock, preferred stock, retained earnings, bonds, and financing without securities.

Two aspects of the organization of the text deserve special mention. First is the heavy emphasis the author places upon internal financing. Second is his treatment of recapitalization, reorganization and expansion. Rather than covering these topics in separate chapters he integrates his discussion of them into the presentation of sources of financing. This approach has the distinct advantage of making the student relate these problems to the types of sources of financing and their characteristics. Unfortunately it also has the disadvantage that a unified treatment of expansion and problems of dealing with financial difficulties is nowhere available.

The book has several interesting features. For example, the author develops the idea of cash flows by means of a series of balance sheets depicting the changes produced by a series of transactions. This treatment, which is quite similar to that employed in developing basic concepts in elementary accounting, is both clear and effective. Throughout, the author has been successful in avoiding the prolonged description of minutiae which all too frequently makes finance texts both lengthy and dull. His literary style is clear and straightforward.

This text can be adapted to either a one- or two-semester course. The addition of extra readings or case assignments would make it suitable for the latter, while a few exclusions can easily pare it down to single-semester proportions. Most of the ideas developed apply to all types of business and are not confined to corporations alone. Consequently the book can be used in any basic business finance course. This is a high-quality text which should be given serious consideration when a finance text is selected.

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Industrial Organization; Government and Business; Industry Studies

The Price Discrimination Law. By CORWIN D. EDWARDS. Washington: The Brookings Institution, 1959. Pp. xxii, 698. $10.00.

This massive work was originally a follow-up study of the effect of lawsuits brought under the Robinson-Patman Act. But the extensive interview material was disappointing, proving again that while interviews are valuable for hints and leads, they cannot form a body of evidence. Unwilling to garnish ignorance with trade opinion and call it research, Edwards made the book a review of the records and decisions in 311 cases, of which 84 were extensively

analyzed. Practicing lawyers in particular will find it useful to have the cases all set out intelligibly, according to the principal legal issues. After a historical sketch and a legislative history, there is a brief synopsis and an analysis largely of procedure but including also the industrial composition of the cases. There follow chapters on brokerage; proportionality in payments and services to buyers; volume, quantity, and functional discounts; territorial price differences, including the abortive campaign for legislation to undo the *Cement* decision; and cases against buyers; "injury to competition," and the defenses of meeting a competitor's price and cost justification; concluding with an appraisal of the Act, and suggestions for legislation.

Edwards' basic method derives from his well-known essay [3]. George Stocking's comment [3] demonstrated that Edwards' "economic power" confused economies of size and monopoly, which two concepts ought to be kept distinct. Edwards' next important work [4] responded to Stocking's criticism by avowedly taking the whole discussion out of the realm of economic analysis; what had been "structural requirements for workable competition" now became "competition, as a political concept," and so on.

The deliberate separation from economics is carried further in the study of a law prescribing price equality. Edwards does not share the illusion that better resource use is a conscious and primary antitrust objective. (Perhaps it should be, but it is not.) Well enough, but in nearly 700 pages there is no economic analysis and no statement of objectives.

Edwards treats economic theory as a set of purposes or values, which it is not, and ignores it as a mechanism for discovering how costs, prices, and outputs are related through a market process. To the "economic idea" that "discrimination exists whenever prices and costs do not vary concomitantly" (p. 2) Edwards contrasts "the political idea," embracing much more than price phenomena, according to which "discrimination is found only in unequal treatment [e.g., unequal prices]. . . . Undue equality is never subject to criticism." But a single example shows this "equality" to be hopelessly ambiguous, and the statement incorrect. "Equal" tax treatment could be lump-sum equality or percentage equality or after-tax equality or some other equality.

Edwards' "equality" therefore is too vague to exist even as a "political" object. By contrast, the economic concept of nondiscrimination, i.e., the complete concordance of prices and costs, is nobody's ideal; it is simply a definite unambiguous point of reference or departure, essential for measuring and appraising facts. If nothing else, it shows that a mere price difference, or a nondifference, means nothing whatever until we can specify a corresponding cost difference or nondifference. A may pay a lower price than B, and yet be discriminated *against*. Nondiscrimination is like a specified latitude and longitude, while "equality" is a point without coordinates. Nothing can be referred to it, and no implications can be drawn from it. Since equal prices charged to A and B can mean discrimination in favor of A, or of B, or no discrimination, a mere equality, or an inequality, without reference to costs, means nothing. "Discriminatory" applied to price differentials is simply a redundancy; yet this is the book's constant usage. Usually, though not always, Edwards has not told us what he is talking about; we are as free to

supply the meaning in each instance as the Greeks consulting the oracle of Delphi, and there is no way of adding up separate facts for a general statement. If "equality" had some definite meaning (such as income equality), reasonable men might well differ as to whether it was worth its cost, but as things stand we have no idea what if anything we are getting from a price equality law, and no standard for appraisal.

Edwards thinks that the Act thwarts efficiency, and he is deeply concerned with the conflict between "efficiency" and "competition," though without explaining either concept, or why there is a conflict. Yet aside from the special case of natural monopolies, where the market is too small to support more than one or very few firms of minimum efficient size, the conflict does not exist; on the contrary, in an unplanned private-enterprise economy competition is the only means to efficiency. Not considering this, Edwards can only evisage an endless discretionary process, of *ad hoc* solutions in particular markets, government "balancing" competition against efficiency, neither concept explained.

The economic theory which Edwards eschews is that, given active competition, the margin over cost on any given product cannot permanently remain greater than on others, for firms will move as soon as they are able from the lower- to the higher-profit activities. With ever-changing supply-demand conditions, new profit discrepancies are constantly being created and dedestroyed. The transitory discriminations are incentives for better resource use, because they occur as part of a competitive process which eventually liquidates them.

A single-firm or group monopoly is free of the compulsion to compete away discriminations, and may be able to maintain them permanently for their greater profit. Hence stable discrimination signals a noncompetitive market and usually wastes resources, particularly when customers are business firms, among whom the less efficient may thus survive.

Two examples of price equality will show the gap left by Edwards' lack of analysis. International airline fares, set by a trade association, are roughly the same per mile on all routes. But the cost per mile is of course very much lower on the heavily traveled North Atlantic run, so that there is really discrimination against passengers on it. "Equality" can be argued either way, without end. But "the economic idea" lets us predict that the airlines will try to get passengers on the North Atlantic runs by every expensive device of advertising or service except the forbidden price-cutting. One would also predict that if any group of passengers could charter the use of a serviceable aircraft, they could travel to Europe at considerably less than conventional rates. And so it has turned out. Substitute the food industry for the airlines; the Robinson-Patman Act, prescribing equal prices, for the international airline cartel; the chain stores and other "powerful buyers" for the clubs and other charterers, and you have it exactly. The key to the facts is the "economic idea" for which Edwards has no use, and the "political idea" does not advance our understanding an iota.

Let us now consider the Robinson-Patman case which Edwards discusses—with his usual sound instinct for what is important—more than any other.

Standard Oil Co. of Indiana sold gasoline at 1½ cents per gallon less to jobbers, who bought in large amounts and provided storage and delivery to service stations, than to retailers, who took delivery on their premises in small amounts. Several jobbers who also operated service stations were included in those who paid the lower jobber price. Under active competition, the price of a given product taken under given conditions will be the same to all buyers, regardless of what they in turn do with it. But Indiana Standard was called upon to "justify" this discrimination on the ground that these integrated customers were retailers, and therefore should by right pay the same price as other retailers. What then happened is cited by Edwards as an example of the "complexities" of ascertaining cost. On the contrary: the complex and exhausting rigmarole served to block the recognition of facts, and it enmeshes every person, however able, who cannot stand off and apply to it those methods of economic analysis which Edwards has renounced.

Outside the world of Robinson-Patman, a half-day's research would have settled the matter. There was no dispute that these customers actually did storage and delivery, and that everyone else buying under the same conditions received a 1½ cent discount. Furthermore, there was uncontroverted evidence that this company saved approximately 1½ cents in other areas by selling to jobbers. A reputable economist [1, pp. 222-27] had faced this very problem of applying to one area storage-delivery cost data from another area, and having shown that it was proper to do so, had estimated the cost as between 1½ and 2 cents. The only conclusion compatible with this evidence is that there was *some* cost saving greater than zero, probably no less than 1½ or more than 2 cents, while the best estimate was 1½ cents. But the ritual of "cost justification," costing thousands of dollars and man-days of effort, picking at trifles and losing the forest not for the trees but for the broken twigs, used the formula "no probative value" to hold that the cost saving was zero. In the contemplation of law, storage and delivery were free gifts of nature.

Edwards is bothered by this, of course, and makes no bones of his general dissatisfaction with "cost justification." (Incidentally, one must also respect his willingness to change his published views materially.) But reflection on "the economic idea" in the light of such cases demonstrates that most if not all cost differentials will simply not be recognized by law, so that many buyers are forced to pay wider profit margins to their suppliers. This is particularly interesting because Edwards would like to get rid of the brokerage clause (2c), which makes no pretense of considering costs but forbids unconditionally any payment or allowance even when the services rendered are obvious, and which therefore leads to unnecessary distribution costs. But if cost justification is largely ineffectual, then the Act as a whole is nothing but the brokerage clause repeated several times. (The Robinson-Patman Act subsidizes higher-cost buyers by not letting lower costs be passed on as lower prices.) Any group of cooperating firms will be under constant tension because they will be tempted to sell a little more of the extra-profit items, and to give away some of the superprofit per unit in order to increase their sales of the item. Hence the sellers must either have a firm understanding, or else there

must be some kind of policing, and this is what the Act provides. The incessant attempts to burrow under it, or wriggle around it, are nothing but competition, or wasteful substitutes therefor.

Edwards might well agree on this last point, for he repeatedly condemns the usual Robinson-Patman standard of "injury to competition" as being nothing but protection of groups of competitors. Yet it cannot be avoided in a price equality law. To this "narrow" view, he opposes the "broad" concept of injury to competition itself, but we never are told what this latter means in theory or practice, and in the end both are in effect rejected: ". . . the objective of establishing equality . . . is not a necessary part of the objective of preserving competition." ". . . the objective of preserving equal opportunity is an inappropriate focus for a law of price discrimination" (p. 641). Intentionally or not, this destroys both "broad" and "narrow" concepts, since both are based on "the political idea" of "equality." And in a remarkably confused footnote (p. 638), he also rejects "a third objective, that of promoting the . . . efficient use of economic resources," apparently because he thinks it would require a sort of universal OPS, neglecting as elsewhere that it is the competitive process, which the law could safeguard, that would push costs and prices into line.

One wonders what Edwards would think if he were not concerned with something called the "large and powerful buyer," heavily emphasized throughout the book. The nearest we get to an explanation is an unbelievably casual "asset size, percentage of the total product bought, or some similar convenient measure" (p. 650). Edwards assembles (pp. 652-53) an indictment of chain stores which can occasionally be understood, but for which he offers no factual support. What these integrated firms did during the 1920's is simple enough as a matter of economics. J. M. Clark [2] pointed out early in the decade that manufacturers tried to help their established dealers "who [desired] protection against direct buying on the part of retailers" [2, p. 415]. The FTC chain store investigation showed that at most some 15 per cent of the chains' lower selling prices was accounted for by lower buying prices; but little—if any—of this could be called discriminatory in economics because the FTC made no allowance in these calculations for the chains' performance of wholesaling, brokerage, or similar functions. Furthermore, the economist in charge of the investigation wrote elsewhere that quantity and related discounts usually did not cover the full cost saving to the seller [5, p. 44]. The only theory to fit these facts is that the chains managed to get lower prices that roughly corresponded to the costs they saved their suppliers. Of course this "aroused the indignation that led to the Robinson-Patman Act"; hell hath no fury like the man deprived of a vested interest. Be this as it may, surely something more is needed than a comparison of chains' absolute sales in 1927 and 1958 (p. 625), with no attention to the rest of the statistical universe, of which they could have been a growing or declining per cent.

"Power" or "bargaining power" or "countervailing power" among buyers shuffles together at least two very different market structures. A buyer with monopsony power (which the chains rarely had) only discriminates among sellers; this problem is outside the present law, and the book. The ability to thwart discrimination, including Robinson-Patman discrimination, or even to

obtain favored treatment, by contacting enough suppliers and offering enough business to make it worth their while to quote lower prices (discriminatory or nondiscriminatory) is simply an economy of scale, and a response to monopoly in selling, without which sellers' discrimination could not exist. We are back, obviously, at Stocking's two categories. Monopoly either in selling or buying can be attacked by law. But if the purpose is "equality," Edwards has himself destroyed its claims to validity. As a standard of public policy, it is, as the lawyers say of a statute, void for vagueness. Hence the chapter appraising the Act says very little: "That it has substantially reduced the discriminatory advantage in price enjoyed by large buyers" (p. 622) may mean that the Act has brought about more or less discrimination. His chief criticism is that the "powerful buyers" have only been attacked indirectly, hence ineffectively; while small buyers banding together have been hurt. Furthermore, the law reached "beyond the problem it was meant to meet" (p. 617), and has protected brokers, promoted wasteful advertising, and damped down competitive vigor. A better law would give more effect to cost differences, and to the "broad concept" of injury.

The Robinson-Patman Act is an economic phenomenon (as well as much else) and must be analyzed as such. But Edwards has renounced the usual analytic apparatus and replaced it with nothing but "power." "Nothing will come of nothing." Stocking's comment in effect warned that meaningless concepts would lead, and they have led, to meaningless results. For all its length, the book is not informative. It is sprinkled with insights reflecting Edwards' impressive experience and knowledge, but this is all frosting without any cake. An acceptable study of the Robinson-Patman Act has yet to be published.

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The Corporation in Modern Society. Edited by EDWARD S. MASON. Cambridge: Harvard University Press, 1959. Pp. xv, 395. $6.75; Text ed., paper, $3.50.

This book is a collection of fourteen new essays contributed by seven economists, three lawyers, two political scientists, a sociologist and a former member of Parliament. The central theme is not the corporation as such but the giant publicly owned concern, its internal control, and its broader social and economic impact. While the essays are independent rather than integrated,

they tend to cluster around a few traditional themes, such as the concentration of economic power, the autonomy of management, and related basic questions that were raised a quarter of a century ago by Berle and Means in *The Modern Corporation and Private Property*. Some of the authors have ranged widely, and the book is broad in perspective, providing insights by students of several branches of the social sciences; the subject matter includes the British corporation, the position of industrial management in Soviet Russia, and the corporate role in international development.

As might be expected, the essays vary considerably in length, quality and content. Some are genuine contributions to our knowledge of the large corporation in its present setting—contributions based on careful and extensive research and scholarly judgment. Others fall short of that standard, and some seem to be largely repetitious of the sweeping generalizations that have plagued the approach to this subject for so long.

It is impossible here to do full justice to all fourteen essays. They cannot be treated as a whole because the subject matter is as diverse as the opinions of the authors. Many seem intent on sounding alarms about the dangers of giant size and all-powerful managements responsible to no one. Most of the alarmist essays dress up old assertions rather than provide a balanced evaluation of them.

In the introductory chapter, E. S. Mason sets the tone of the book as he expresses concern, or even fear, because the large corporation, a permanent part of our economy, is subject to no visible controls that will insure operation in the public interest. He distrusts even the favorable verdict of experience, fearing that the generally good performance of the economy to date might have been fortuitous, offering little assurance for the future. He takes small comfort in the fact that concentration has not been growing in the past 50 years, since absolute size has grown and that, together with irresponsible management, is the nub of the problem. Much of what he has to say is debatable, in light of available evidence, but the issues are forcefully presented.

Several of the essays are first-rate and represent valuable additions to our much-needed knowledge of how the large corporation really behaves. Among these is John Lintner's highly competent research study of corporate financial policies during the past half century, with particular emphasis on the Berle and Means theses of increasing concentration, and how large corporations escape the discipline of the capital markets through internal financing. These theses he finds questionable. The proportion of outside borrowing has been growing in recent years for most industrial groups, and large manufacturing corporations are now more dependent on outside funds than they were thirty years ago. Moreover, the rate of expansion and shifts between bond and stock financing have been pretty much consistent with a market-oriented economy rather than with the hypothesis of arbitrary and autonomous management power.

Jacob Schmookler's essay on technological progress and innovation will also be welcomed as a contribution based on extensive research. He finds that a surprising amount of applied research and development is carried on by individuals and small concerns, with over 85 per cent of the 15,000 industrial

concerns doing research having fewer than 500 employees each. He doubts the need for giant enterprises to stimulate technological development, and he finds research an ally of small enterprise that tends to keep the economy competitive. He questions the wisdom of present patent laws and urges more government support for basic scientific research.

W. Lloyd Warner's carefully documented study of 8,300 top corporation executives dissipates the popular image of the "organization man" who owes his position to circumstances of birth or social connections, and is a mere cog in a bureaucratic machine, with little imagination or initiative. Apparently there is an increasing degree of mobility up and down the executive scale and between enterprises, with more apparent emphasis on ability than hereditary position or connections.

C. A. R. Crosland's essay on the private corporation and the public corporation in Great Britain is both descriptive and analytical. He detects little evidence of increasing concentration in British private industry in the past half century (although British industry is more concentrated than American), and he sees no dangerous degree of private power there. The theory of the public corporation, with its autonomy of operation and final accountability to Parliament, has not in either respect been fully realized in practice.

The position of management in Russian industry is the subject of an informative essay by Alexander Gerschenkron. There have been swings of the pendulum of official policy between extreme rigidity and some measure of managerial freedom. He sees real dangers of such freedom to the dictatorship and for that reason expects the recent policy to be reversed.

Raymond Vernon's essay on some timely international aspects of the business corporation in its relation to underdeveloped areas explores another facet of big business. After examining needs and present tendencies, he concludes that only if business and government join forces are we likely to provide the know-how and capital that are necessary to meet the Russian challenge.

Carl Kaysen's chapter on the power of big business does not get past the stage of assertion. He implies that power is in proportion to the range of choice, and he measures concentration by the indiscriminate lumping together of the public utility, transportation, financial, manufacturing, and mining industries which are responsible for 51 per cent of the national income. He leaves the questionable impression that a few corporate giants exercise almost unlimited power over most of the leading industries, if not the entire economy, creating economic inefficiency, instability, impediments to progress and inequities. He repeats most of the cliches about the power of big business over foreign policy, its control of the press, and its dictation of policies to supine state and local governments. There is none of the careful analysis of market structures or the plain facts of political life that a useful approach to this subject requires. The essay is essentially the brief of the attorney for the prosecution.

In sharp contrast to Kaysen's presumed all-powerful corporation is Neil Chamberlain's portrait of labor unions as essentially conservative and benign, with little monopoly power (or seldom using it), and having few basic con-

flicts with management (except to impress the membership). Prosperity rather than union power is the explanation of wage increases in recent years; and unions are mostly interested in job security, fringe benefits, grievance procedures, and the like. Certainly it is difficult to reconcile the concepts of arbitrary power and responsibility in these two chapters.

The essays by lawyers and political scientists are in the main related to the internal affairs of the corporation, and the distribution, derivation, and use of power. Abram Chayes finds the corporation so all-powerful in modern society that it must be made subject to the rule of law. Presumably it is constrained neither by competition, by stockholders, nor by countervailing power. He suggests more control over corporate directors by labor unions, dealers, and others, rather than by stockholders. Stockholders presumably have the least need for protection—after all, they can sell their stock! Chayes does not pursue the economic implications of his suggestions.

Kingman Brewster, Jr., explores analogies between corporations and sovereign governments and finds that the former do not have the checks and balances necessary to the democratic process. He would limit corporate size to the requirements of economic efficiency to gain the advantage of dispersion in decision making, thus preserving the social value of the small concern in a sort of "economic federalism."

In a well-reasoned essay, Eugene V. Rostov correctly notes that stockholders' influence or control over management is by no means a rarity, although they do not use their franchise as effectively as the political citizen. With refreshing insight he perceives the dangers of the "new capitalism" in which public-conscious management conceives itself as responsible to everyone—stockholders, workers, customers, dealers, and all—and no longer seeks to maximize profits. He concludes that, both in law and economics, management is likely to perform best when it follows traditional profit motivation.

Earl Latham's efforts to squeeze the large corporation into the mold of the sovereign state appear strained, over-drawn and unrealistic, although some similarities do exist and have been long noted.

Norton Long contributes a realistic essay on the place of the corporation in the local community. He suggests that managers have become sensitive to local needs and are jealous of good public relations, avoiding the older positions of dominance and paternalism. On the whole, they are likely to have little local political influence—much less than labor unions and other groups.

These brief comments indicate the range of subject matter and quality found in the essays. In the quarter century that has passed since the publication of Berle and Means, a large number of useful and competent studies of the large corporation and concentrated economic power have appeared. Many of these cast serious doubts on the validity of some of the sweeping generalizations of that challenging book. Moreover, as Berle notes in his foreword to this new volume, many new laws and regulations have altered in some degree the power of the large corporation and its management. Yet, much of the advance seems to be inadequately reflected in some of the essays.

Students of corporate enterprise would welcome a new synthesis that embraces the research of the past thirty years. This is not that synthesis; al-

though some of the essays, without doubt, would qualify as contributions to it. More than half of the volume deserves a wide audience both in college classes and among the general public. And even those parts of the remainder that seem inaccurate, oversimplified, or unconvincing contain thought-provoking ideas that provide springboards for discussion. It is not basically a book for beginners, who need something more systematic and who lack the background to separate the wheat from the chaff. It can be used to real advantage for reference or as a supplementary textbook where thought-provoking ideas are more important than widely accepted conclusions.

A word must be said about the quality of the writing: Almost without exception, it is excellent for this type of book. The style is crisp, direct, brief, and sometimes brilliant. If the book was designed to stimulate controversy, the issues are presented with vigor. But it would have been improved by more emphasis on objectivity and balance.

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Integration and Competition in the Petroleum Industry. By MELVIN G. DE CHAZEAU and ALFRED E. KAHN. Petroleum Monograph Series, Volume 3. New Haven: Yale University Press, 1959. Pp. xviii, 598. $7.50.

The petroleum monograph series which is sponsored by a grant from the American Petroleum Institute finally contains a work of solid merit. Whereas the first two items in the series[1] were generally disappointing, in this third volume de Chazeau and Kahn have made an important contribution to literature concerning the oil industry. Their book is a detailed and penetrating analysis of vertical integration in that industry; in addition, it essentially redoes a good deal of the work attempted in the first two volumes—on pricing and conservation—and does it better.

The broad questions posed for analysis are whether the present form of organization of the petroleum industry is essential to provide an adequate supply of petroleum products now and henceforth, and whether the observed massive vertical integration and horizontal concentration are compatible with the competitive objectives of our society. More specific general inquiries concern whether this integration is a necessary and desirable consequence of the characteristics of the industry, whether integration and concentration have gone further than efficiency requires, what are the conditions of workable competition in petroleum, and whether the American proration system is the best road to conservation. These latter questions suggest that the book is to be a good deal more than a narrow study of integration, and this is indeed the case; it is effectively a study of operation of large integrated firms in the petroleum industry, and of the industry in which they operate. In the authors' words, "this study represents a search for judgment as to whether the combination of public controls, private policy, and competition in the petroleum

[1] Ralph Cassady, Jr., *Price Making and Price Behavior in the Petroleum Industry*, 1954; E. W. Zimmermann, *Conservation in the Production of Petroleum*, 1957. Both volumes have been reviewed in this journal—the Cassady book, Sept. 1954, *44*, 691-95 and the Zimmermann, Dec. 1958, *48*, 1066-67.

industry has produced and is likely to continue to produce socially acceptable behavior and results."

In spite of the breadth of their general inquiry, however, the authors do repeatedly come back to the narrower question of how much difference does vertical integration per se really make with respect to this or that phase of industry behavior. The reviewer found the attempts to answer this question in various contexts slightly less satisfactory than other parts of the book, perhaps mostly because the question itself is somewhat akin to those of how much difference the hydrogen makes in an organic compound built up of hydrogen, carbon, and oxygen, or how much difference the front wheels make to an automobile.

The book is broken into four main parts, of which the first is introductory and deals with the general structure of the industry and with the economic and historical setting of its vertical integration. Part II is concerned broadly with crude oil production and conservation and their relation to integration; and in a series of excellent chapters discusses regulation of oil production by state authorities (prorationing), the pricing of crude oil in the domestic market, its pricing in the world market, and the role of imports in the making of domestic production and pricing policies.

Part III considers the alleged advantages of vertical integration in the "non-price" areas of policy and competition, with especial attention to the planning and execution of investment and innovation at various levels in the vertical sequence of industry operations. Part IV analyzes competition in the refined-product markets and the effects of vertical integration thereon, giving considerable attention to the impact of size *cum* integration on product prices and to the competitive places of nonintegrated, "independent" refiners and marketers. It concludes with an all too brief summary chapter concerning the over-all effect of vertical integration on petroleum-industry competition and concerning the general workability of this competition.

Covering a great deal of territory in less than 600 pages, the book would not be expected to be, and is not, one based upon and reporting results of extensive primary research. It is based, however, on an extremely comprehensive reading—and, better yet, a good critical understanding—of voluminous relevant secondary materials. A great deal thus depends on the general analytical ability and good judgment of the authors. Although the reviewer occasionally disagreed with their judgment, he would rate them high on both counts. And he was especially impressed both with their ability to cut through the trivia in order to concentrate on essential issues, and with their objective, dispassionate, and unbiased approach to a range of quite controversial questions.

The book is so packed with factual and interpretive content (it definitely "reads long") that it is only possible in a brief review to suggest its flavor by characterizing a few leading conclusions. As to the reasons for integration, the authors conclude after extended discussion that whereas early vertical integration by the old Standard Oil (up to 1911) had a fairly clear monopoly motive, the major rationale of integration since has been stabilization of markets and prices, or a defense by the firm against inherent market instabilities, and

the capture by the firm of secure and dependable sources of supply for refineries and dependable outlets for their products. Both competitive and monopolistic considerations enter here. Since the introduction of prorationing in the 1930's, dampening of the instability of crude supplies and prices has become less important, but integration of secure crude supplies has become more so. Except with reference to the early days of the industry, the authors pay relatively scant heed to possible efficiency-increasing effects of vertical integration in oil, and certainly do not assign it a major role in the explanation of such integration.

The historical emergence and characteristics of the control of oil output through state prorationing are nicely covered in a very sophisticated chapter. The strategic role of Texas oil and the Texas regulatory commission in stabilizing American domestic oil supply and raising and stabilizing crude oil prices is made extremely clear, as is the indirect influence of the integrated major oil companies on the policies and decisions of the Texas commission. As to the domestic pricing of crude oil, arguments to the effect that oil prices must and do respond in any necessary or systematic way to the long-run costs of finding oil are considered and appropriately rejected; instead it is emphasized that the interaction of the decisions of the Texas commission and the market power of the major integrated firms essentially makes and stabilizes American crude oil prices at a quasi-arbitrary level.

The making of foreign oil prices, their shifting relationship to Texas Gulf prices, and the predominant role of integrated-firm market power plus political considerations in fixing world oil prices and determining imports to the United States are explained. And it is lamented that the pivotal Texas price is more influenced by a casual consideration of domestic cost than by a competent appraisal of the world demand-supply situation. Crude-oil integration by large oil firms in conjunction with domestic production control is considered a bad thing. The integrated majors think like producer-sellers rather than buyer-sellers of crude; there is a corresponding lack of a strong bona fide buyer interest in proration deliberations; and the result is an undue pressure to elevate domestic crude prices and maintain them at elevated levels. Meanwhile, untoward conservation results ensue: proration is clearly inferior to the rejected alternative of compulsory unitization, and the policy of drawing heavily on expensive domestic oil while shutting off cheap foreign imports is poor conservation without convincing national-defense justifications. Unitization instead of proration, and an abolition of quota limits on imports, are favored.

The section on investment and innovation in relation to integration is much less satisfactory than the extremely strong one on crude oil. It is concluded on balance that integration may favor progressiveness in technique and product, and encourage adequate pipeline investments, although it does perhaps implement the incursion of excessive costs of distribution and selling. The section as a whole, though sensible, is strongly taxonomic in its approach, unduly general, and rather inconclusive in its outcome.

The ensuing discussion of product markets and prices is in general quite competent. It is appropriately recognized that "spot" markets have been

systematically overrated and that major-company tankwagon prices are the leading instrument of price policy—also that whereas price leadership in making these prices for gasoline may be somewhat barometric, the prices arrived at are not necessarily purely competitive ones. Nonetheless, there is a considerable degree of intermajor competition in the product markets, for which the incompleteness or imbalance of the integration of many majors must be thanked. (Over-all balanced integration would probably be anticompetitive in the net.) The current general picture is that existing price-making in the product markets does not engender monopolistic margins or raise product prices much farther than they have already implicitly been pushed by crude output control under prorationing. Following this discussion, there is a rather perceptive analysis of the evolving roles and declining numbers of independent refiners, ending with the observation that unitization without proration, plus freedom of oil imports, would do a great deal to restore and sustain the competitive position of firms in this category.

In general conclusion, the authors feel that there is "a workable balance between monopolistic and competitive aspects of this industry's structure and performance—beyond the crude oil level." Integration per se is not held to be the real culprit in the case against monopolistic tendencies; in any event, monopoly power stems from horizontal size rather than vertical integration. The blame must be placed on prorationing, the Texas regulatory commission, the policy with respect to oil imports, and the operation of integrated firms in areas much broader than existing regulatory jurisdictions. These things combine with integration, and especially crude-oil integration by the major refiners, to engender quite unsatisfactory results. But the remedy of vertical divestitures should be rejected, or at least postponed until the preferred remedy of eliminating prorationing, instituting unitization, and allowing free imports has been given a fair trial. These are broad and sweeping conclusions, but they have been adequately qualified in the supporting chapters.

The reviewer has a few quarrels with the authors. For example, insufficient attention is given to the possibility that the major firms may elevate refinery-distribution margins by systematic price discrimination among various types and grades of refined products; and the over-all policy prescription, while attractive, is not necessarily very practical. In general, however, this is a strong and important book, which should at once please the experts and be especially appreciated by students who want to learn a great deal about an important industry in a short time.

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Le pouvoir de monopole. Essai sur les structures industrielles du capitalisme contemporain. By JACQUES HOUSSIAUX. Paris: Sirey, 1958. Pp. iv, 416. 3,800 fr.

Professor Houssiaux's essay on monopoly power consists of two books. Each book is divided into two parts (*titres*) and each part is further divided into two chapters and a number of sections. Monopoly power (*pouvoir de monopole*) is defined as the resultant of two elements: the economic power of the

firm (*puissance économique*) and its behavior pattern (*comportement* or *action stratégique*). The first element is determined by the degree of concentration. The most important ingredients of the second element, the firm's behavior pattern, are: price behavior, decisions with regard to variety and quality of products, attitude towards innovations, sales efforts (*comportement publicitaire*), and conditions of entry (pp. 2-3, 65-71). The *comportement* thus is the exercise of its economic power by a firm. Only the conditions of entry do not quite seem to fit into the list. What Houssiaux apparently has in mind is the firm's decision to use its economic power in order to restrain potential newcomers from entering the market. For the purpose of empirical investigation of economic structures and of monopoly power, the author falls back on measurements of the degree of concentration. This he justifies by an extensive analysis of the relation between the degree of concentration and the monopoly power of the firm which indicates that on the whole monopoly power varies directly with the degree of concentration.

The first book (177 pp.) deals with theoretical systems of market classification and with measures of market imperfection in terms of demand elasticities and cross elasticities (Part I); and with the concepts and the methods of measuring concentration (Part II). It is a very complete and painstaking survey of the major contributions to monopoly literature of M. Adelman, J. S. Bain, R. Bishop, E. Chamberlin, W. Fellner, N. Kaldor, A. P. Lerner, A. Papandreou, K. W. Rothschild, R. Triffin, *et al.* This analytical digest of a quarter century's discussion will be of great value to French economists unable to read English. To the English-speaking economist it has little to offer. Incidentally, this very fact raises the question of the near-monopoly position held by English and U. S. writers on monopoly power. Houssiaux apparently fails to see that there is a problem which is at least indirectly relevant to his investigation.

The second book is devoted to an empirical study of the degree of monopoly in the French economy, supplemented by a comparison of the development of the French and the U. S. industrial structures since the beginning of this century. It is quite independent of the first book. Houssiaux states (p. 181) that his empirical study "requires methods of research entirely different" from those of the first book, without, however, indicating why this is so. At any rate, these methods consist essentially of simple statistical measures of concentration, historical description, and some case-study techniques. Judged by itself, the second book is an excellent investigation of the present state and of the evolution of competitive conditions in France. Part I is devoted to a comprehensive verbal description of market structures and competitive conditions in the main sectors of the French economy (Ch. 1); and to a broad statistical analysis of the prevailing degree of concentration by firms and by plants (*établissements*) using number of employees, gross revenue and gross assets as criteria (Ch. 2). Chapter 1 of Part II is concerned with the historical evolution of effective monopoly power in France and—for the purpose of comparison—in the United States. The examination of the major relevant developments is subdivided into a verbal discussion of the critical factors determining monopoly power (size, number, and interde-

pendence of markets, innovations, technologically determined size of the enterprise, institutional framework) and a statistical analysis of the changes which have occurred in the competitive conditions in France (1912-52) and in the United States (1899-1947). This analysis proceeds by measuring changes in the degree of concentration by industrial sectors and industries using value added (for the United States), output, gross assets and changes in the composition of the group of the 100 largest firms as main criteria. The final chapter contains a detailed, well-documented investigation of the role and the effect of mergers on monopoly power and a statistical study of the merger movement in France (1919-1954) and the United States (1887-1947).

There is nothing really new in the methods of the analysis. What is new and of great value is the systematic and careful application of these methods to all available French statistics and to supplementary descriptive evidence. The investigation indicates that unlike the industrial structure in the United States and in England, the French economy is still largely dominated by quite small enterprises: technological concentration has been slower here than in other industrialized countries. On the other hand, monopoly power measured in terms of concentration on the firm, rather than the plant (*établissement*) level has increased in France since the beginning of the century (while during the same period it has remained stable or has even declined in the United States). In the light of these findings Houssiaux formulates the interesting hypothesis that it is precisely the lag of technological concentration which has led in France to horizontal and vertical integration; that is, to the emergence of large firms owning or otherwise controlling groups of small enterprises, and to collusive arrangements. In this development mergers have played an important part. In order to make the French economy more competitive, the author suggests that technological concentration must be stimulated, so that the "most dynamic enterprises may grow to a size consistent with the most efficient production" (p. 296). In addition he recommends, of course, stricter legislative and administrative controls.

By raising the question of why the technological concentration in France has lagged so far behind that in other industrial countries, Houssiaux might have provided his results with a firmer understructure. Indeed, this is one of the central questions of French economic development since the 18th century and possibly since the gold and silver inflation of the 16th and 17th centuries. But an author should not be blamed for the limits which he chooses to impose on a study and for leaving some work for others to do. As it stands, Houssiaux's general hypothesis offers a number of new and important insights into the processes which formed and continue to form the structure of the French economy.

The major defects of this study are in the form of presentation. The book is poorly organized, and the writing is often verbose and repetitious. The total absence of cross-references and of a summary of major conclusions in a work of this length puts a heavy burden on the reader. There is no index. There is also an excessive number of misprints, some of them quite disturbing. On the asset side, there is an excellent and very complete bibliography of

the literature on monopoly. Whatever the irritating defects of the presentation, Houssiaux's study, and in particular its second book, represents an important and welcome contribution to monopoly literature.

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Die Eisenbahntarife im Güterverkehr. By NORBERT KLOTEN. Tübingen: J. C. B. Mohr (Paul Siebeck), 1959. Pp. 224. DM 24.80.

This book is the thirteenth volume of the publications of the List Gesellschaft in Basel, Switzerland. Under the editorship of E. von Beckerath and E. Salin, this society has published a number of outstanding studies concerning a variety of important contemporary economic problems..

Mr. Kloten's book is one of the few publications originating in postwar West Germany about railroad rates. The author attempts to establish a theoretical foundation for the determination of railroad rates; but little is said about policies actually pursued by railroads. The American reader who wishes to learn, for reasons of comparison, how railroad rates are actually made in West Germany may therefore be slightly disappointed.

In the first of three parts, "Foundations," the author deals initially with production and cost structure of railroads, emphasizing especially such features as simultaneous, alternative and joint production. Several pages are devoted to the well-known problem of fixed cost; some cost data are furnished, mainly referring to the former German *Reichsbahn* or the *Bundesbahn* of the Federal German Republic. (Throughout the book the author refers to German railroads, though a few interesting international comparisons are made.)

Part I then turns to demand—first the problem of demand for transport in general—then specific features of the demand for railroad transport. Considerable attention is given to the price elasticity of railroad transport demand. The problem of the relation between the level of national income and the levels of demand for transport in general and railroad transport in particular is mentioned but not investigated in detail. Part I closes with an attempt to explain the market behavior of railroads in terms of the von Stackelberg classification of market forms (which the author follows closely). The author recognizes, however, that market behavior may be conditioned not only by the nature of the market form, ranging from perfect competition over oligopoly to monopoly, but also by considerations of "the general public interest" as well.

Part II is lengthy and deals in great detail with railroad rates, their structure, classification and particular characteristics. The author thoroughly investigates what the conceptual elements of railroad rates are (*Elementar Tarife*) and how these can be combined into a rate system. The approach in this part is highly taxonomic and less interesting for the economist who looks rather for meaningful relationships than for new definitions and precise classifications.

Part III, dealing with "rate systems" (railroad rates proper), is more interesting. After some comments on older German concepts about railroad rates

(with reference to the writings of F. Ulrich and E. Sax), the features of a rate system which aims at maximizing profits are discussed. The analytical tools are those of conventional marginal analysis. Using a number of simplifying assumptions and with some resort to *ceteris paribus*, the author shows under what conditions a railroad—which can split the demand for its services—can maximize profits.

He suggests that an alternative aim of a railroad may be to cover total cost. If this should be done in a socially optimal way (assuming competition) price must be equal to marginal cost and average cost. It is assumed that average cost is falling and that it will rise steeply once the existing equipment is used nearly to capacity. A railroad will not under normal circumstances—for lack of a large enough demand for its transport services—use its equipment to such an extent that marginal cost is equal to average cost. Operating in the range of falling average cost poses then the problem how to cover the gap between marginal and average cost. A possibility would be to maintain marginal-cost pricing and subsidize the railroad through the budget, which is the theoretically appealing solution analyzed by Hotelling. The author does not go into this but investigates the more practical way of setting rates higher than marginal cost. The condition imposed is that total cost must be covered with a minimum average rate level. To achieve this it is necessary to split the demand for transport services according to categories of goods transported and "charge what the traffic will bear."

After the discussion of these two basic alternatives, some of the stringent assumptions are relaxed and the author deals in a descriptive way with the problem of competition of other carriers (inland waterway shipping and trucks). The book ends with observations on locational implications of railroad rates.

Kloten's book reflects a desirable revival of the interest of German economists in problems of railroad rates. His application of conventional marginal analysis to problems of railroad transportation is, considering the state of traditional German theories about railroad rates, a laudable endeavor; but he is fully aware of the limitations of the marginal analysis in this field.

A few remarks may be made about the author's contention (p. 171) that, from the point of view of the total economy, no rate system is as favorable as that which attempts cost coverage through minimum average rate levels by means of price discrimination. The fact that a railroad can cover cost—especially if this is done through price discrimination—evidently does not enable us to make statements about the quality of resource use and general welfare. To substantiate his position the author would have to supply the difficult proof of the proposition that price discrimination improves both resource allocation and general welfare. It is a pity that he did not investigate the problem of marginal-cost pricing and subsidization of railroads in detail. If this had been done and due attention been paid to excise tax features of a railroad rate, the author might have reached a somewhat different conclusion regarding a most favorable rate system.

A general criticism of the analysis is that it is presented in too narrow a conceptual framework. There is too much emphasis on the railroad as a firm

and too little on its important position in and its impact upon the total economy. But the analytical content of the book is the better part. Less inspiring are the history of ideas about railroad rates. The reader is exposed to a variety of concepts, classifications and definitions offered by various writers (most of them German) in this field. In the opinion of this reviewer this could have been much shorter without loss to the really important things the author has to say.

All chapters are well documented and there is an extensive bibliography of the important German writings in this field.

KARL W. ROSKAMP

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Land Economics; Agricultural Economics; Economic Geography; Housing

Economics for Agriculture: Selected Writings of John D. Black. Edited by JAMES PIERCE CAVIN. Cambridge: Harvard University Press, 1959. Pp. v, 719. $12.00.

The main feature of this book is: it pays tribute to a person who made tremendous contributions to the development and knowledge of agricultural economics in particular and to the subject of economics in general. As a tribute to John D. Black's contribution, the book was indeed timely. His life ended shortly after publication.

Black covered an extremely wide range of subjects and analyses in his writing and teaching. The book has been organized accordingly. It includes selections of his writings not only in the individual fields of specialization in the broad area of agricultural economics, but also in political science, economic theory and world economic development. The book has been divided into thirteen parts, each including some of Black's writing in the particular area. The first part includes a portrait of J. D. Black written by J. K. Galbraith in his most lucid style. Each subsequent part includes an introduction by one or more of Black's former graduate students.

The areas covered include: development of production economics, economics of land use, economics of forestry, labor and tenure, marketing and cooperation, price analysis, consumption and nutrition, population and world food supply, agriculture in the national economy, agricultural policy, political science and the public interest, research methods and economic theory. In addition to an introduction by one of his former students, each part includes two chapters from Black's writings. The introductory chapter of each part includes an analysis of the setting and time when Black did the writing, with an interpretation of Black's interest and contribution. But the introductions extend beyond these interpretations and summarize developments in research and methodology in the particular subject-matter areas. For example, in Part II on production economics, Sherman Johnson and Kenneth Bachman begin with the origin of farm management by Spillman, Hayes, Boss and Warren. After discussing the research tools used by these persons, they discuss the trend towards use of economic principles by such persons as Black, Taylor,

Ely and others. They summarize the reorientation of production economics research in the 1920's, its modification during the war and depression and the "new ferment" coming from younger workers relying more on basic economic and econometric tools in the postwar period. Johnson and Bachman also discuss some of the problems in research and methodology to be anticipated in the future.

In the introductory chapter on economics of land use, to take another example, Marion Clawson reviews the development of concepts in and definitions of the field. He discusses important variables affecting land use and devotes a short section to the contribution of Black. In the part on consumption and nutrition, Willard Cochrane traces Black's contribution here, as well as the thinking of persons such as Sir John Boyd Orr, H. R. Tolley, Margaret Reid and others. Similar broad treatments of Black's contributions and development of thought in particular fields are included in the introductory chapters by Black's former students who served as editors.

Most of the selections from Black's work are from journals and other articles which contain ideas not presented by him in his numerous books. These articles, plus the interpretations given them, often relate to modern economic problems of agriculture, as well as to the problems which existed at the time of the original interpretation. Numerous problem solutions suggested by Black are being suggested at the present time, thus indicating the permanence of both problems and alternatives in their social treatment as well as the fundamental nature of analyses by Black.

Fortunately this tribute to John D. Black was gathered together before his death. No other person will likely contribute as much to a particular field of economics as he did. However, it is much more than a tribute. The numerous editors not only pulled together a systematic group of Black's papers, but also summarized, in an excellent way, important developments in agricultural economics, research methodology, and progress towards solution of important and continuing problems.

EARL O. HEADY

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Toward a New Energy Pattern in Europe—Report of the Energy Advisory Commission, O.E.E.C. Paris: Organisation for European Economic Cooperation, 1960. Pp. 125. $1.50.

This report is the latest analysis of the Western European energy situation sponsored by the O.E.E.C. Based on a study carried out by an international group of experts headed by Professor Austin Robinson, it succeeds the well-known Hartley Report, now almost four years old. In the tradition of energy studies, the Robinson Report forecasts future requirements for energy and then considers how these needs may be met. The empirical estimates serve as basis for policy recommendations which are, of course, the real heart of the Report.

Economic expansion in Europe will be accompanied by further rapid growth of energy consumption. The forecasts, approximately a 30 per cent increase

in primary energy consumption from 1955 to 1965 and again from 1965 to 1975, are of the same general magnitude, slightly higher in fact, as the estimates of the Hartley Commission, despite the Suez crisis and the recession which have intervened. Indigenous European energy supplies are also growing but it is difficult to forecast quantitatively which sources of the energy will actually be drawn on. The Hartley Commission, working in a setting of energy shortage, could estimate the indigenous production potential and then view the residual as an import gap. But now the problem has become one of competition. Energy, domestic as well as foreign, is in surplus and more and more consumers have been turning to that source of supply which is economically most advantageous.

With regard to supplies, the Robinson group deals with broad magnitudes. It assumes a growing but limited potential for hydroelectric power, and somewhat more rapid development of indigenous natural gas and indigenous petroleum than had previously been foreseen. Nuclear power, which will not be competitive until the 1970's on the basis of a realistic appraisal of its capital cost, will until then make only a small contribution to the total energy supply. The coal industry, until recently the focus of European efforts to boost indigenous energy output, now finds itself with a large oversupply. The problem of coal is basically one of high cost, and steps to improve the position of the industry by closing marginal mines are consistent with some reduction in its output.

This evaluation of indigenous energy leads the Robinson group to anticipate a sizeable and growing need for imports, principally of crude oil and some coal, though natural gas presents a visionary but yet not fully tested potential. These imports, amounting perhaps to 32 per cent of European energy consumption in 1965 and 39 per cent in 1975, will be considerably greater even than had been forecast by the Hartley Commission. The latter expressed considerable concern about this "energy gap" and urged the rapid development of indigenous sources of energy. The Robinson Report, on the other hand, views the growing European import requirements for energy as a normal development, not significantly more serious than similar "gaps" in food supplies, in textile materials, etc. The Robinson group does not see any danger of a persistent long-term shortage of energy. It finds that Europeans will be able to pay for their vast energy imports and that security may be better achieved by drawing on a diversity of supply sources than by development of high-cost indigenous resources.

This means simply that Europeans should consume that energy which costs least, that interferences with pricing and other restrictions on energy use should be eliminated, and that the full costs of developing indigenous sources of energy should be recognized. Only two exceptions are considered. One is the case of the coal industry which requires time and financial aid to effect a technological and social transition. The other is the limited development of commercial nuclear energy which may be viewed as a purchase of knowledge in a new technology.

What then are the critical changes which account for the reorientation in

point of view reflected by the Robinson Report? Certain significant changes have indeed occurred: (1) There has been a realization that there is a surplus of energy. Large new discoveries of petroleum and natural gas have been made recently, but even prior to these discoveries, reserves and potentials were known to be very extensive. In fact, the plentiful availability of energy has become apparent on the market place. Imported fuel oil and coal are now in many parts of Europe cheaper than indigenous energy. (2) New discoveries of oil and gas outside the Middle East have reduced the dependence of European consumers on one, politically vulnerable, source of supply. (3) The balance of payments of most European countries is now greatly improved.

These changes in the setting of the European energy economy, considered in some detail in the Report, are essential to its policy recommendations. But the Report is not simply the result of these changed circumstances. On the basis of the same evidence, other groups have argued for increased protection of domestic resources. Protectionism in the field of energy is far from a dead issue in Europe. The Robinson Report is a welcome expression of liberal principles at a time when Europeans are giving serious thought to energy policy and to its coordination.

F. Gerard Adams

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Copper Costs and Prices: 1870-1957. By Orris C. Herfindahl. Baltimore: Johns Hopkins Press, for Resources for the Future, 1959. Pp. xi, 260. $6.00.

The author has investigated in detail the annals of the copper industry for a period of almost 90 years. He wanted to test the generalizations of mineral economics by learning whether the accidental discovery of new sources of mineral wealth and government subsidies for development have resulted in discontinuity in introducing new productive facilities or whether the processes of investment are systematic and continuous, responding to the signals of profit and loss like competitive nonmineral industries.

Direct determination of costs is of course difficult because accounting data often conceal the costs of discovery of new deposits. The author has therefore chosen to approach the subject indirectly by the use of deflated prices. He argues that in the absence of collusion, competitive prices are equivalent to costs. To eliminate periods of noncompetitive prices, he undertakes an examination of the degree of competition and concentration in the industry—an illuminating study to which he devotes about half the volume. According to his findings the periods of collusion are clearly separated from the periods of competition. He proceeds then to develop a table of historical prices, classifying individual years as normal or abnormal and considering as normal the years in which there is competitive pricing. From the normal years he eliminates not only years in which price manipulation is known to have occurred but also years in which war or depression exercised extreme influences on demand or supply.

Even if the author's methods made it possible to eliminate individual years

as not normal—and this remains open to question—economists may well argue that the residual prices are not equivalent to costs in the sense the author intends. Nor does his method throw light on the behavior of profits and losses.

The elimination of abnormal years is a problem because war and depression are not coterminous with the calendar. They result from complicated antecedent causes and leave behind an inheritance which changes the nature of succeeding events. Similarly, despite much convincing evidence that concentration in the copper industry has resulted from changes in leadership of the big units, it is hard to agree that the author is quite secure in characterizing individual years as competitive or noncompetitive. The nature of evidence about competitive practices in the United States makes it difficult to interpret; industry spokesmen are necessarily less than candid about the price-making process. Although he has weighed this evidence with care and attention to detail, his conclusions must be looked at as tentative, representing a novel approach to a difficult problem.

Accepting the author's method and assuming it is possible to pick out individual years that are normal in his meaning, we have in the whole span of 90 years, only 49 he believes can be called normal. Even within the 49 are 7 which he does not exclude although they involve the operation of Copper Exporters (1926-1928) and the international cartel (1936-1939); he judges these two episodes to have had but a limited effect on price. The 49 years also include 14 based on London prices, the argument being that during these 14 years United States prices were dominated by the Michigan combine. Of the 49 years, excluding the 14 and the 7, none comes after 1925. For the last year, 1957, the author uses, in lieu of "cost," a range of what he considers to be representative industry opinion of an adequate long-run price.

Difficult as it is to characterize a great industry with neat generalizations, the present detailed examination of the periods of price manipulation, the behavior of various organizations of producers, the measurement of the degree of concentration—including comparisons with other industries, and the shifts in geographical distribution and rank of producers, add importantly to our understanding of the push and pull of actual industry. For example, in the period of 90 years, there has been substantial change in the sources of the world's copper supply; new areas have come into production as old areas have tapered off. Efforts of organized producers have encountered serious obstacles in deciding upon an agreed basis for restriction of production because of the changing constituency of national producers and even of individual companies. During periods of collusion, producers characteristically fail to control additions to producing capacity so that they have never exercised enough control to increase prices substantially. In general then price manipulation has not been cumulative but discontinuous and episodic.

In looking at the history of deflated copper prices, the author concludes that prices have been relatively stable. Important technological improvements about the time of the first world war resulted in substantial economies which were passed on to consumers. Had it not been for a declining rate of growth in the demand for newly mined copper, the deflated long-run price of copper

would have increased after the first world war. This decline in demand resulted from the increasing importance of copper recovered from old scrap and the large decline in the relative price of aluminum. The conclusion is that discovery, development, and production of copper are systematic. While there may be accidental discoveries of new sources, they occur in a field which is so broad that their influence does not dominate. The estimate of long-run price is high enough (and near enough to present levels) to attract sufficient exploration and bring into production new areas as needed. If we may judge by projecting past experience, we need have no fear that we shall shortly run out of copper.

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Labor Economics

Labor in a Free Society. Edited by MICHAEL HARRINGTON and PAUL JACOBS. Berkeley and Los Angeles: University of California Press, 1959. Pp. xi, 186. $3.00.

This book is the result of a conference held in May 1958 at Arden House, Harriman, N.Y., under the sponsorship of the Basic Issues Program of the Fund of the Republic. Its theme was apparently suggested by the then pending plans for new federal legislation to regulate the unions, plans which have since become realized in the Landrum-Griffin Labor Reform Law of 1959.

Opening with a foreword by Clark Kerr which stresses the importance of clarifying basic social issues through many-sided discussion, the book contains seven papers which were delivered at the conference, followed by a brief summary of the principal points made in the discussions.

As to the papers, three were given by men from the academic ranks in the United States: psychoanalyst and author Erich Fromm, the late Harvard economist Sumner Slichter, and Archibald Cox of the Harvard Law School; two others by experts from different walks of the practice of labor relations in the United States: David A. Cole, one-time director of the Federal Mediation and Conciliation Service, and Arthur J. Goldberg, counsel for the United Steel Workers of America; finally two more by citizens of the British Commonwealth: Hugh A. Clegg of Oxford University, and James R. McClelland, Australian labor attorney.

With the exception of the paper by Fromm ("Freedom in the Work Situation") which raises—though naturally does not answer—broad questions as to the meaning of "freedom" and its role among other social and human values, all papers deal quite specifically with the problem of whether and in what respects legal regulation of trade unions is desirable. Summing them up briefly and all-too-incompletely: Slichter ("The Position of Trade Unions in the American Economy") finds, in part, that collective bargaining by the powerful unions which have developed in the United States may eventually need government regulation unless unionism is capable of transcending itself in the direction of broader social goals. Cox ("The Role of Law in Preserving Union

Democracy"), insisting on the need of legally safeguarding the rights of individuals and minorities in the interest of sound unionism, appraises existing and proposed legislation for the regulation of internal union policies. Cole ("Union Self-Discipline and the Freedom of Individual Workers") warns against facile analogies between political and union democracy, claiming that, with collective bargaining the primary purpose of unions, the rights of individual workers, are not under all circumstances a prime consideration. As to dealing with corrupt union leadership, he thinks that legally to strengthen the powers of the AFL-CIO federation might be wiser than to increase direct governmental powers. Goldberg ("A Trade-Union Point of View") argues against all government regulation of internal union affairs which go beyond the "setting of limits to injustice"; regulations which try to do more than this are likely to hinder the growth of unionism in the many areas where it is still very weak, and altogether constitutes an essential loss of freedom to society. Clegg ("The Rights of British Trade-Union Members") asserts that for a variety of historical and sociological reasons the internal policies of the British unions do not seem to be in need of government regulation; while McClellan ("Experiences of the Australian Labor Movement under Government Control") argues that in Australia labor-management relations in general and internal union policies in particular are and must remain subject to the democratic governments—which in fact are often labor governments—of that country.

It is evident that the collection of these papers with their many-sided, well-argued views based on thorough knowledge of facts and rich personal experience, and with many additional points of view brought out in the discussions, will be most valuable to any reader who wants to form an intelligent opinion about these problems, and of particular value also as reading material in connection with courses in labor legislation, trade union developments, and similar ones.

As regards the "Basic Issues Program," however, this symposium seems to prove that the question of legal regulation of trade unions in a free society is really not a basic issue but rather one of expediency only ("expediency," according to Webster, is "that which is suitable to the ends in view"). Given the basic freedoms of a democratic society, the extent and nature of governmental regulation of trade unions can and do vary greatly with economic, social, and historical cimcumstances: they do differ widely as between free Great Britain, free Australia, the free United States, and still other free countries; and they also differ widely between the United States of 1935, of 1960, and presumably of 2000 and beyond.

More truly basic issues which may come to be increasingly in need of clarification, on the other hand, might be those which remained more or less peripheral at this conference: the question of whether "freedom" is necessarily an absolute social value, or whether it is meaningful only in the context of other, more basic issues such as the purposes, human and social, of a society.

JOHN V. SPIELMANS

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Understanding Labor Problems. By DALLAS M. YOUNG. New York: McGraw-Hill, 1959. Pp. xii, 477. $7.95.

Professor Young states in the preface that his book "has been written for anyone who wants to understand labor problems. It is an attempt to present professionally sound material in a clear, informal, and somewhat conversational style" (p. ix). He further claims that if one is a supervisor the book is designed for him, that if one is a labor leader the book is written for him, and that if one is a college student the book will provide him with "some insight."

Apparently the only people for whom the book was not written are professors of labor economics or professionals in the field of labor relations. Young candidly admits that if one is either or both he knows "far more than is found herein" (p. x).

It is perhaps not fair to review such a book in a professional journal unless the reviewer keeps in mind the book's intended audience. Even keeping this limitation firmly in mind, however, I am not sure that Young's approach is the best one for achieving the commendable end he has in view. The author wishes to interest uninformed students in labor problems. He also wishes his book to serve as a tool for training relatively uninformed line supervisors and union leaders. His method of accomplishing these objectives is to alternate massive doses of verbatim factual materials (i.e., texts of laws, contracts, arbitration decisions, etc.) with chatty conversational sections, ostensibly designed to make the reader think for himself in various roles in problem situations. The book is virtually devoid of serious analytical discussion on the author's part.

This reviewer does not believe that one can intrigue reader interest at either professional or subprofessional levels by addressing him always as "you" and coming perilously close in many of these chatty interludes to "talking down" to said reader. I think Young may have underestimated the reader. If he has not done so, then the level of information and understanding on the part of the claimed audience for this book is distressingly, if not disastrously, low.

The book's organization is reasonably conventional. It begins with a section devoted to exhaustive definitional analysis of such terms as "labor" and "labor problems," followed by a brief historical survey of the growth of American unions and employers' associations. Parts 3 and 4 constitute the heart of the book, containing respectively a fairly detailed analysis of current issues in collective bargaining and the history and current content of basic labor legislation, including the text of the Labor Management Reporting and Disclosure Act.

Part 5 is a factual summary of pertinent state and federal legislation in such areas as social security, unemployment insurance and minimum wage legislation. Part 6 is another factual section on National War Labor Board and National Wage Stabilization Board experience. The concluding section is provocatively entitled "Where do We go from Here?" Unfortunately, no attempt is made to answer the posed question. We are simply reminded that the whole book deals with the United States and we must be aware that labor problems exist elsewhere in the world. No bibliographies or suggestions for additional

reading have been included for the benefit of the reader whose interest might have been aroused.

When he is not quoting verbatim at length from some law or contract, Young writes well and objectively. He also displays some humor on occasion, a rare commodity in the field of labor economics and labor relations. Young is an experienced arbitrator. Thus, as one might expect, the section dealing with collective bargaining problems and grievance arbitration is the most rewarding in the book, both from an informational and an analytical standpoint. In other sections, the author appears to be less sure of himself and takes refuge in heavier injections of quoted materials.

This book may well enjoy a considerable vogue as a "practical" and objectively prepared "tool" for use in workers' education or foreman training programs. From an academic or professional standpoint, however, its contribution must regretfully be regarded as minimal.

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NOTES

SEVENTY-THIRD ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Chase Park Plaza Hotel, St. Louis, Missouri—December 28-30, 1960

Preliminary Announcement of the Program, July 15, 1960

The sessions are organized around the broad theme "Frontiers of Economic Knowledge."

Tuesday, December 27, 1960

6:00 P.M. EXECUTIVE COMMITTEE DINNER MEETING

Wednesday, December 28, 1960

9:30 A.M. MONETARY THEORY: NEW AND OLD LOOKS

Chairman: ELI SHAPIRO, Massachusetts Institute of Technology
Papers: JAMES TOBIN, Yale University
JOHN KAREKEN, University of Minnesota
KARL BRUNNER, University of California, Los Angeles
Discussants: To be announced

ANTITRUST PROBLEMS

Chairman: EDWARD S. MASON, Harvard University
Papers: The Anti-Merger Act, 1950-1960
M. A. ADELMAN, Massachusetts Institute of Technology
Policy Implications of A Theory of Inter-Firm Organization
ALMARIN PHILLIPS, University of Virginia
Mergers and Cartels: Some Reservations about Policy Trends
DONALD J. DEWEY, Duke University
Discussants: JAMES W. MCKIE, Vanderbilt University
REUBEN E. SLESINGER, University of Pittsburgh
JEROME B. COHEN, College of the City of New York

2:30 P.M. ECONOMIC ANALYSIS OF URBAN PROBLEMS (Joint Session with Regional Science Association)

Chairman: SHERMAN J. MAISEL, University of California, Berkeley
Papers: Intra-Urban Location Theory
CHARLES M. TIEBOUT, University of California, Los Angeles
Contrasts in Agglomeration—New York and Pittsburgh
BENJAMIN CHINITZ, University of Pittsburgh
Pricing Policies in Urban Redevelopment
LOUIS WINNICK, New York State Commission on Economic Expansion
Discussants: BARBARA R. BERMAN, Harvard University
BRITTON HARRIS, University of Pennsylvania
IRVING MORRISSETT, Purdue University

PUBLIC UTILITIES AND TRANSPORTATION

Chairman: BEN W. LEWIS, Oberlin College
Papers: Fully Distributed Costs in Utility Ratemaking
JAMES C. BONBRIGHT, Columbia University

Evaluation of Statistical Accounting as Applied to Transportation Industries
JOHN R. MEYER, Harvard University
GERALD KRAFT, Harvard University
Discussants: H. THOMAS KOPLIN, University of Oregon
DANIEL MARX, JR., Dartmouth College
RICHARD A. TYBOUT, Ohio State University

FRONTIERS IN UNCERTAINTY THEORY—THE EVIDENCE OF FUTURES MARKETS (Joint Session with Econometric Society)
Chairman: D. GALE JOHNSON, University of Chicago
Papers: An Introduction to Behavior Analysis
HOLBROOK WORKING, Stanford University
Systematic and Random Elements in Short-Term Price Movements
HENDRIK S. HOUTHAKKER, Harvard University
Common Elements in Futures Markets in Commodities and Bonds
PAUL COOTNER, Massachusetts Institute of Technology
Discussants: RUTH P. MACK, National Bureau of Economic Research
MICHAEL BRENNAN, Brown University
MARC NERLOVE, Stanford University

8:00 P.M. PRESIDENTIAL ADDRESS
Chairman: PAUL H. DOUGLAS, United States Senate
Presidential Address: THEODORE W. SCHULTZ, University of Chicago

Thursday, December 29, 1960

9:30 A.M. ECONOMIC DEVELOPMENT IN MAINLAND CHINA
Chairman: FRANKLIN L. HO, Columbia University
Papers: National Income of the Chinese Mainland 1958-59
TA-CHUNG LIU, Cornell University and
K. C. YEH, The RAND Corporation
The Statistical System of Mainland China With Particular Reference to Agriculture
CHOH-MING LI, University of California, Berkeley
Sino-Soviet Economic Relations: A Reappraisal
ALEXANDER ECKSTEIN, University of Rochester
Discussants: FRANKLYN D. HOLZMAN, University of Washington
SIDNEY KLEIN, Rutgers University

MACRO-ECONOMIC THEORIES OF INCOME DISTRIBUTION
Chairman: MARTIN BRONFENBRENNER, University of Minnesota
Papers: Conflicting Views on Relative Shares
MELVIN W. REDER, Stanford University
Real versus Monetary Theories of Income Distribution
SIDNEY WEINTRAUB, University of Pennsylvania
Distributional Effects of Alternative Monetary and Fiscal Policies
OSWALD H. BROWNLEE, University of Minnesota and
ALFRED H. CONRAD, Harvard University
Discussants: ROBERT W. OZANNE, University of Wisconsin
BORIS P. PESEK, Michigan State University

CAPITAL THEORY
Chairman: ROBERT DORFMAN, Harvard University
Papers: Implications of Capital Theory for Economic Development Programs
OTTO ECKSTEIN, Harvard University
Implications of Capital Theory for Corporate Investment Decisions
JACK HIRSHLEIFER, University of Chicago
Implications of Capital Theory for Statistical Measurement of Capital and Wealth
JOHN W. KENDRICK, George Washington University

Discussants: FRANCIS M. BATOR, Massachusetts Institute of Technology
VERNON L. SMITH, Purdue University
ZVI H. GRILICHES, University of Chicago

12:30 P.M. ECONOMIC ADJUSTMENTS AMONG NATIONS (Joint luncheon with American Finance Association)

Paper: Economic Adjustments Among Nations
THEODORE O. YNTEMA, Ford Motor Company

2:30 P.M. ECONOMIC EDUCATION: CHALLENGE TO OUR PROFESSION

Chairman: ARTHUR F. BURNS, Columbia University
Papers: This is Economics in the Schools
PAUL R. OLSON, State University of Iowa
This is Economics
HOWARD S. ELLIS, University of California
The Responsibility of the Profession
G. L. BACH, Carnegie Institute of Technology
Discussants: G. SHOREY PETERSON, University of Michigan
E. T. WEILER, Purdue University

WHEAT: A PERMANENT NEED FOR A GOVERNMENT PROGRAM (Joint Session with American Farm Economics Association)

Chairman: G. E. BRANDOW, Pennsylvania State University
Papers: Titles to be announced
HELEN C. FARNSWORTH, Food Research Institute, Stanford University
JOHN A. SCHNITTKER, Kansas State University
Discussants: O. H. BROWNLEE, University of Minnesota
L. E. FOURAKER, Pennsylvania State University
CALVIN B. HOOVER, Duke University

PROBLEMS OF ECONOMIC INSTABILITY IN OTHER COUNTRIES

Chairman: R. A. GORDON, University of California, Berkeley
Papers: Postwar Developments in the Scandinavian Countries
ERIK LUNDBERG, University of California, Berkeley
Growth and Stability in the Postwar Italian Economy
GEORGE H. HILDEBRAND, Cornell University
Business Cycles in Postwar Japan
SHIGETO TSURU, Hitotsubashi University
Discussants: GARDNER ACKLEY, University of Michigan

8:00 P.M. INVITED LECTURE

Chairman: LLOYD G. REYNOLDS, Yale University
Paper: The *General Theory* After Twenty-Five Years
HARRY G. JOHNSON, University of Chicago
Discussants: ALVIN H. HANSEN, Harvard University
DAVID MCCORD WRIGHT, McGill University
ABBA P. LERNER, Michigan State University
LAWRENCE R. KLEIN, University of Pennsylvania

RUSSIAN WAGES (Joint Session with the Industrial Relations Research Association)

Chairman: ABRAM BERGSON, Harvard University
Papers: Recent Developments in the Soviet Wage Structure and the Work of the Wage Commission
WALTER GALENSON, University of California, Berkeley
Comparative Developments in Wage Structure of the Steel Industry in the Soviet Union, Poland, Yugoslavia and Western Countries
GARDINER CLARKE, Cornell University
Discussants: EMILY C. BROWN, Vassar College
HAROLD M. DOUTY, Bureau of Labor Statistics

Friday, December 30, 1960

9:30 A.M. THE BALANCE OF PAYMENTS OF THE UNITED STATES: PROBLEMS AND PROSPECTS

Chairman: CHANDLER MORSE, Cornell University

Papers: Disturbances and Adjustments in Recent U.S. Balance of Payments Experience
HAL B. LARY, National Bureau of Economic Research

Unbalanced International Accounts: Diagnosis and Therapy
J. HERBERT FURTH, Board of Governors of the Federal Reserve System

The Adequacy of United States Gold Reserves
E. M. BERNSTEIN, E.M.B. (Ltd.)

Discussants: JAMES BURTLE, W. R. Grace and Company
JAROSLAV VANEK, Harvard University

ECONOMICS AND NATIONAL SECURITY

Chairman: T. C. SCHELLING, Harvard University

Papers: The Propensity to Reduce The National Debt Out of Defense Savings
EMILE BENOIT, Columbia University

Which Industries Would Be Most Important in a Postwar U.S. Economy?
DONALD V. T. BEAR, Stanford University
PAUL G. CLARK, Williams College

Strategy in Active Defense
W. THORNTON READ, Bell Telephone Laboratories

The Interrelationship of Strategic Objectives
DANIEL ELLSBERG, The RAND Corporation

The Economics of Stand-By Capacity for Military Emergencies
ALLEN R. FERGUSON, Northwestern University

DISTRIBUTION COSTS—CONCEPTS AND MEASURES (Joint Session with American Marketing Association)

Chairman: WILLARD W. COCHRANE, University of Minnesota

Papers: The Effects of Distribution Costs on Demand
LESTER TELSER, University of Chicago

Specialization, Scale and Costs in Retailing
RICHARD H. HOLTON, University of California, Berkeley

An Interpretation of Changes in Agricultural Marketing Costs
F. V. WAUGH, U.S. Department of Agriculture, and
KENNETH OGREN, U.S. Department of Agriculture

Discussants: WARREN J. BILKEY, University of Notre Dame
REAVIS COX, University of Pennsylvania

2:30 P.M. THE INFLUENCE OF MORAL AND SOCIAL RESPONSIBILITY ON ECONOMIC BEHAVIOR

Chairman: LELAND J. GORDON, Denison University

Papers: The Influence of Moral and Social Responsibility on Advertising and Selling Practices
COLSTON E. WARNE, Amherst College

The Influence of Moral and Social Responsibility on Executives of Large Corporations
ERNEST DALE, Cornell University

The Influence of Moral and Social Responsibility on Selling Consumer Credit
ARCH TROELSTRUP, Stephens College

Discussants: HOWARD M. TEAF, JR., Haverford College
RAYMOND T. BYE, University of Pennsylvania
DEXTER M. KEEZER, McGraw-Hill Publishing Co.

THE CASE OF INDIA

Chairman: WILFRED MALENBAUM, University of Pennsylvania

Papers: Development Horizons for India
GEORGE ROSEN, United Nations Secretariat
The Strategy of Planning with Special Reference to the Third Plan
DON D. HUMPHREY, Fletcher School of Law and Diplomacy
Performance and Plan: Analysis of the Gap
ASHOK MITRA, International Bank for Reconstruction and Development
Discussants: WILLIAM W. HOLLISTER, Washington, D.C.
CHARLES WOLF, JR., The RAND Corporation

MANAGERIAL ECONOMICS: A NEW FRONTIER?
Chairman: MERTON H. MILLER, Carnegie Institute of Technology
Papers: The Current State of Managerial Economics
W. W. COOPER, Carnegie Institute of Technology
What Can Economic Theory Contribute to Managerial Economics?
WILLIAM J. BAUMOL, Princeton University
What Can Managerial Economics Contribute to Economic Theory?
CHARLES J. HITCH, The RAND Corporation
Discussants: JULIUS MARGOLIS, University of California, Berkeley
ALAN MANNE, Yale University
FRANCO MODIGLIANI, Northwestern University

5:00 P.M. BUSINESS MEETING

6:00 P.M. EXECUTIVE COMMITTEE DINNER MEETING

NEW PUBLICATIONS

The first issue of *Yugoslav Survey*, a quarterly journal in English, was published in April 1960. The *Survey* contains documentary and informative articles on the economic, social and cultural life of Yugoslavia. Communications about the journal should be sent to Jugoslavija Publishing House, Nemanjina 34, Belgrade, Yugoslavia.

The International Training and Research Center for Development, IRFED, is now publishing a quarterly review *Développement et Civilisations* devoted to problems of undeveloped countries. Communications and manuscripts should be sent to the editor, Madeleine Trebous, 29, Place du Marché-Saint-Honoré, Paris 1er, France.

Announcements

Creation of a National Task Force on Economic Education has been announced by Theodore W. Schultz, president of the American Economic Association and by Donald K. David, chairman of the Board of the Committee for Economic Development, which is sponsoring the undertaking. The primary objective of the Task Force is to attempt to define what economics high school students should and can be taught for effective citizenship and participation in our democratic system.

Professor Schultz has made the following appointments to the group: George L. Bach, Carnegie Institute of Technology, chairman; Lester V. Chandler, Princeton University; Robert A. Gordon, University of California, Berkeley; Ben W. Lewis, Oberlin College; and Paul A. Samuelson, Massachusetts Institute of Technology. Recommendations will represent their independent views and not the views of the American Economic Association, the Committee for Economic Development or any other special economic group.

The Social Science Research Council's annual announcement describing fellowships and grants to be awarded in 1960-61 is now ready for distribution. It lists the following pro-

grams to be continued without major changes: research training fellowships, faculty research fellowships; grants-in-aid of research; grants for research on national security policy. Under joint sponsorship with the American Council of Learned Societies, grants are to be offered to mature scholars for research in the social sciences and humanities on certain foreign areas.

Applications for some categories of awards will be due not later than November 1. Inquiries should be addressed to the Social Science Research Council, 230 Park Avenue, New York 17, N.Y.

Economists from about 40 nations will meet in Geneva, Switzerland in the summer of 1961 for an International Conference on Input-Output Techniques. The meeting will take place in the Palais des Nations from August 28 through September 1, 1961. The Conference is being organized by the Harvard Economic Research Project of Harvard University in association with the United Nations Secretariat and is supported with funds from the National Science Foundation and the Rockefeller Foundation. Wassily Leontief, Henry Lee Professor of economics at Harvard University, will be chairman of the conference. Dr. Elizabeth Gilboy, lecturer on economics at Harvard, will be secretary general.

The Fund for Social Analysis is again offering a limited number of grants-in-aid for studies of problems posed by Marxist theory and its application. Grants will ordinarily range from $500 to $3,000. Address the Corresponding Secretary, The Fund for Social Analysis, Room 2800, 165 Broadway, New York 6, N.Y.

Deaths

Carl R. Bye, Syracuse University, May 23, 1960.

Fred C. Croxton, Washington, D.C., April 3, 1960.

Heinrich W. Lück, Godesberg, Germany.

Archibald M. McIsaac, Syracuse University, January 11, 1960.

Curtis H. Morrow, May 7, 1960.

Edwin E. Witte, emeritus, University of Wisconsin, May 6, 1960.

Harry D. Wolf, University of North Carolina, June 8, 1960.

Retirements

Thomas A. Budd, Wharton School, University of Pennsylvania, June 1960.

Paul J. FitzPatrick, Catholic University of America.

Floyd B. Haworth, University of Illinois, September 1960.

David B. Jeremiah, Wharton School, University of Pennsylvania, June 1960.

A. L. Lomax, University of Oregon.

Elinor Pancoast, Goucher College, September 1960.

Wilbur C. Plummer, Wharton School, University of Pennsylvania, June 1960.

Robert Riegel, University of Buffalo, July 1960.

Visiting Foreign Scholars

Bruno Leoni, University of Pavia: distinguished visiting scholar, Thomas Jefferson Center for Studies in Political Economy, University of Virginia, fall term, 1960.

Erik Lundberg, University of Stockholm: visiting research professor of economics, University of California, Berkeley, 1960-61.

Robin Marris, Kings College, Cambridge University: visiting professor of economics, University of Texas, fall semester; lecturer in economics, spring semester 1961, University of California, Berkeley.

Hla Myint, visiting professor of economics, Cornell University, spring term, 1961.

J. N. Wolfe, University of Toronto: visiting professor of economics, University of California, Berkeley, 1960-61.

William Woodruff, Melbourne University: University of Illinois, 1960-61.

Promotions

Edward Ames: professor of economics, School of Industrial Management, Purdue University.

E. J. Ball: professor of business administration, University of Arkansas.

Gary S. Becker: professor of economics, Columbia University; Ford research professor, 1960-61.

Joseph W. Conard: professor of economics, Swarthmore College.

Paul G. Darling: professor of economics, Bowdoin College.

R. Kirby Davidson: professor of economics, School of Industrial Management, Purdue University.

Robert R. Dince: associate professor, College of Business Administration, University of Georgia.

Norton T. Dodge: assistant professor of economics, University of Maryland.

Richard A. Easterlin: professor of economics, Wharton School, University of Pennsylvania.

Otto Eckstein: associate professor of economics, Harvard University.

Robert R. Edminster: associate professor of economics, University of Utah.

Eber W. Eldridge: associate professor of economics and sociology, Iowa State University (Ames).

Frederic N. Firestone: assistant professor of economics, Wellesley College.

Leslie Fishman: associate professor of economics, University of Colorado.

Gilbert L. Gifford: professor of economics, University of Arizona.

John G. Gurley: professor of economics, University of Maryland.

Howard H. Hines: professor of economics and sociology, Iowa State University (Ames).

A. M. Huq: associate professor of economics, University of Vermont.

Donald P. Jacobs: associate professor of finance, School of Business, Northwestern University.

Alexandre Kafka: professor of economics, University of Virginia.

Mark L. Kahn: professor of economics, Wayne State University.

John W. Kendrick: professor of economics, George Washington University.

Clifton H. Kreps: Wachovia Professor of Banking, School of Business Administration, University of North Carolina.

Harold C. Krogh: professor of business administration, University of Kansas.

Harvey Leibenstein: professor of economics, University of California, Berkeley.

John M. Letiche: professor of economics, University of California, Berkeley.

E. E. Liebhafsky: professor of economics, Agricultural and Mechanical College of Texas.

F. Ray Marshall: professor of economics, Louisiana State University.

Edwin S. Mills: associate professor of economics, Johns Hopkins University.

John R. Moore: professor of business administration, University of Tennessee.

Richard R. Newberg: professor, department of agricultural economics and rural sociology, Ohio State University.

Hugh S. Norton: professor of transportation, University of Tennessee.

Arthur M. Okun: associate professor of economics, Yale University.

Robert C. Ortner: lecturer on statistics, Wharton School, University of Pennsylvania.

James P. Payne, Jr.: professor of economics, Louisiana State University.

Joseph S. Peery: assistant professor of economics, University of Utah.

A. A. Pepelasis: associate professor of economics, University of California, Davis.

Boris P. Pesek: associate professor of economics, Michigan State University.

Frank S. Pinet: associate professor of business administration, University of Kansas.

Stanley Reiter: professor of economics, School of Industrial Management, Purdue University.

V. W. Ruttan: professor, agricultural economics department, Purdue University.

Wilson E. Schmidt: professor of economics, George Washington University.

Leon M. Schur: associate professor of economics, Louisiana State University.

J. T. Scott: associate professor of economics and sociology, Iowa State University, (Ames).

Richard E. Shannon: associate professor of economics, Montana State University.

Edgar T. Shaudys: associate professor, department of agricultural economics and rural sociology, Ohio State University.

John W. Skinner: associate professor of economics, George Washington University.

Gerald G. Somers: professor of economics, University of Wisconsin.

Jack D. Steele: professor of business administration, University of Kansas.

George Strauss: professor, school of business administration, department of industrial relations, University of Buffalo.

Ronald L. Stucky: professor of industrial management and associate dean, School of Industrial Management, Purdue University.

Milton C. Taylor: professor of economics, Michigan State University.

D. W. Thomas: professor of agricultural economics, Purdue University.

Erik Thorbecke: associate professor of economics and sociology, Iowa State University (Ames).

Charles M. Tiebout: associate professor of economics, University of California, Los Angeles.

Thomas A. Yancey: associate professor of economics, University of Illinois.

Wesley J. Yordon: assistant professor of economics, University of Colorado.

Administrative Appointments

D. E. Armstrong: professor and director of the School of Commerce, McGill University.

Floyd Bond: dean, School of Business Administration, University of Michigan.

Sol S. Buchalter: chairman, department of finance, San Fernando Valley State College.

James D. Calderwood: chairman, department of business economics and international trade, School of Business Administration, University of Southern California.

Robert G. Cox: vice dean, undergraduate division, Wharton School, University of Pennsylvania.

Maurice E. Dance: chairman, department of economics, San Fernando Valley State College.

John S. Day: associate dean, School of Industrial Management, Purdue University.

Lowell E. Gallaway: director, Bureau of Business Services and Business Research, San Fernando Valley State College.

William F. Hellmuth: dean, College of Arts and Sciences, Oberlin College.

Donald D. Humphrey: director, William L. Clayton Center of International Economic Affairs, Fletcher School of Law and Diplomacy, Tufts University.

C. Clyde Jones: chairman, department of business administration, Kansas State College.

E. W. Kierans, McGill University: president of the Montreal and Canadian Stock Exchanges.

William B. Keeling: director and professor, Bureau of Business Research, University of Georgia.

Hal B. Lary: associate director of research, National Bureau of Economic Research.

Stephen L. McDonald: head, department of economics, Louisiana State University.

John Nordin: acting head, department of economics, Iowa State University (Ames), 1960-61.

Raymond Pelissier: director, School of Business Administration, Georgetown University.

Edward S. Shaw: acting executive head, department of economics, Stanford University, 1960-61.

John W. Skinner: acting executive officer, department of economics, George Washington University.

Daniel L. Spencer, Southern Illinois University: professor of economics and head of department of economics, Howard University.

Sidney Weintraub: chairman, department of economics, Wharton School, University of Pennsylvania.

Richard M. Westebbe, Board of Governors, Federal Reserve System: executive director, Foreign Trade Administration, Ministry of Commerce, Greece.

Nathaniel Wollman: chairman, department of economics, University of New Mexico.

Appointments

Arthur T. Andersen: instructor, department of economics, Boston University.

Y. B. Awh: assistant professor of economics, Nichols State College, Louisiana.

Thomas R. Beard: assistant professor of economics, Louisiana State University.

Philip W. Bell, University of California, Berkeley: associate professor of economics, Haverford College.

David Bodenberg: instructor in economics, Princeton University.

Norman M. Bradburn: assistant professor of behavioral sciences, Graduate School of Business, University of Chicago.

George F. Break, University of California, Berkeley: member of senior staff, Brookings Institution, for special research.

Ayers Brinser: professor of economics, University of Colorado.

J. E. Brown, University of Florida: assistant professor of economics, University of Washington.

Conrad Caligaris: assistant professor of business and economics, University of Maine.

Robert W. Campbell: visiting associate professor, University of California, Berkeley, fall term; associate professor of economics, Indiana University, beginning February 1961.

George K. Chacko: analyst, operations research section, Atlas Powder Company.

Robert J. Connor: instructor in production management, Graduate School of Business, University of Chicago.

Ward S. Curran: instructor in economics, Trinity College.

Michael F. Dacey: assistant professor of regional science, Wharton School, University of Pennsylvania.

Ernest M. DeCicco: instructor, department of economics, Boston University.

Harold Demsetz: assistant professor, department of economics, University of California, Los Angeles.

W. Stanley Devino: assistant professor of business and economics, University of Maine.

Donald Dewey, Duke University: associate professor of economics, Columbia University.

Peter O. Dietz: instructor in finance, School of Business, Northwestern University.

Lev E. Dobriansky: professor of economics, Georgetown University.

Peter Drucker: visiting professor of industry, Wharton School, University of Pennsylvania.

I. M. Drummond: lecturer, department of political economy, University of Toronto.

James N. Duprey: staff of economics department, University of North Dakota.

Carl T. Eakin, University of Notre Dame: associate professor of marketing, University of Georgia.

Edward C. Fei, formerly of Swarthmore College: associate professor of economics, University of Wisconsin.

William J. Fellner: Ford research professor, Columbia University, 1960-61.

Lehman B. Fletcher, University of California, Los Angeles: assistant professor of economics, Iowa State University (Ames).

Hugh W. Folk: lecturer in economics, University of California, Berkeley, 1960-61.

D. F. Forster: lecturer, department of political economy, University of Toronto.

Karl A. Fox, Iowa State University (Ames): visiting professor of economics, Harvard University.

Harry L. Franklin: visiting lecturer, department of economics, Georgetown University.

Daniel R. Fusfeld, Michigan State University: appointment at University of Michigan.

Lloyd L. Gallardo, Michigan State University: appointment at St. Mary's College, California.

Jamshed Ghandhi: lecturer in finance, Wharton School, University of Pennsylvania.

Herbert A. Goertz: instructor in economics, Dartmouth College.

Raymond W. Goldsmith: professor of economics, Yale University.

H. A. J. Green: assistant professor, department of political economy, University of Toronto.

James L. Green, Wright-Patterson Air Force Base: professor of economics, College of Business Administration, University of Georgia.

Karl D. Gregory: assistant professor of economics, Wayne State University.

George F. Hadley: associate professor of business administration, Graduate School of Business, University of Chicago.

George D. Hanrahan, University of Minnesota: assistant professor of economics, Saint Louis University.

Alvin H. Hansen, Harvard University: fellow, Center for Advanced Studies, Wesleyan University, second semester, 1960-61.

Hugh G. Hansen: visiting associate professor of economics, Cornell University, 1960-61.

D. G. Hartle: assistant professor, department of political economy, University of Toronto.

Rex D. Helfinstine, formerly Agricultural Research Service, U.S. Department of Agriculture: staff of economics department, South Dakota State College.

W. J. Herman: instructor in business administration, University of South Florida.

Donald D. Hester: instructor in economics, Yale University.

Forest G. Hill, University of Buffalo: professor of economics, University of Texas.

Alan Hoffman: associate visiting professor, New School for Social Research.

Hendrik S. Houthakker, Stanford University: professor of economics, Harvard University.

Hans J. Jaksch, formerly University of Frankfurt/Main: assistant professor of economics, The Rice Institute.

C. Hayden Jamison, Beloit State Bank: lecturer in economics, Beloit College, 1960-1961.

Sidney L. Jones: assistant professor of business administration, School of Business, Northwestern University.

S. J. Kagan, Joint Council on Economic Education: faculty of School of Business Administration, University of Oregon.

Reuben A. Kessel: assistant professor of business economics, Graduate School of Business, University of Chicago.

Robert E. L. Knight, University of New Mexico: assistant professor of economics, University of Maryland.

Frederick E. Kottke: assistant professor of economics, University of Southern California.

Aranka E. Kovacs: lecturer in economics, McGill University.

Sigmund Krauthamer: assistant professor of economics, University of Utah.

Marian Krzyzaniak, Montana State University: lecturer 1960-61, The Johns Hopkins University.

Simon Kuznets, The Johns Hopkins University: professor of economics, Harvard University.

Richard A. LaBarge, Southern Methodist University: financial analyst, Ford Motor Company.

Alexis E. Lachman, International Cooperation Administration, Rome: financial advisor to Finance Minister of Laos.

William E. Laird, Jr.: assistant professor of economics, Florida State University.

Norman Levine: research economist, Systems Research Group, Mineola, L.I.

Fritz Machlup, The Johns Hopkins University: professor of economics and director, International Finance Section, Princeton University.

William L. McDaniel, Princeton University: assistant professor of economics, University of New Mexico.

Raymond H. McEvoy, Montana State University: economist, Bank of America National Trust and Savings Association, San Francisco.

David McFarland: lecturer in economics, Princeton University.

Jack Melitz: fellow at Thomas Jefferson Center for Studies in Political Economy, University of Virginia.

E. D. Milenev: assistant professor of economics, Hartwick College.

Richard A. Miller: instructor in economics, Wesleyan University.

Jacob Mincer: associate professor of economics, Columbia University.

John B. Miner, University of Pennsylvania: associate professor, School of Business Administration, University of Oregon.

Harry A. Miskimin, Jr.: instructor in economics, Yale University.

Aurelius Morgner: associate professor of economics, University of Southern California.

John G. Myers: assistant professor of economics, University of Colorado.

H. R. Neville: visiting professor of economics and management, Louisiana State University, first semester, 1960-61.

Alan T. Nichols: assistant professor of economics, School of Business, University of South Dakota.

Alfred Parker: instructor in economics, Oklahoma State University.

Jan Parker: instructor in economics, Wellesley College.

Hugh T. Patrick: assistant professor of economics, Yale University.

Edmund S. Phelps, Jr.: assistant professor of economics, Yale University.

Gerald A. Pinsky: instructor in economics, Dartmouth College.

Sher J. Rana, University of Alaska: assistant professor of economics, University of Puerto Rico.

Gustav Ranis: assistant professor of economics, Yale University.

James R. Ratliff: assistant professor of accounting, School of Business Administration, University of Pittsburgh.

Ira J. Rees: assistant professor of business administration, University of Georgia.

Dennis Reinmuth: staff of economics department, University of North Dakota.

Richard A. Ridilla, University of Pittsburgh: position with the Pittsburgh National Bank.

Gaston V. Rimlinger, formerly Princeton University: associate professor of economics, The Rice Institute.

J. Thomas Romans: lecturer, department of economics, University of Buffalo.

Stephen W. Rousseas: visiting associate professor of economics, University of Maryland, 1960-61.

David J. Saposs: lecturer in international labor relations, School of International Service, The American University.

Wolfgang Schoellkopf: instructor in economics, Cornell University, 1960-61.

M. C. Schnitzer, University of Florida: staff of Virginia Polytechnic Institute.

David Schwartzman: associate professor, department of economics, New School for Social Research.

Martin Shubik: visiting professor of economics, Yale University.

Ronald E. Simmons: assistant professor of economics, DePauw University.

Rudolph Skandera: assistant professor of accounting, College of Business Administration, University of Georgia.

John H. Smith: professorial lecturer in statistics, Graduate School of Business, University of Chicago.

Eugene Smolensky, University of Pennsylvania: assistant professor of economics, Haverford College.

Richard C. Spangler, Montana State University: research post, University of Hawaii.

Case Sprenkle: assistant professor, department of economics, University of Illinois.

Thirukodikaval N. Srinivasan: instructor in economics, Yale University.

Barry Supple: associate professor of economic history, McGill University.

Izumi Taniguchi, University of Texas: assistant professor of economics, University of Missouri.

Daniel Thorner, Indian Statistical Institute, Bombay: associate professor, Ecole Pratique des Hautes Etudes, Sorbonne, Paris, 1960-61.

S. G. Triantis: associate professor in the department of political economy, University of Toronto.

Jan V. Tumlir: instructor in economics, Yale University.

Hirofumi Uzawa: assistant professor of economics and mathematics, department of economics, University of California, Berkeley.

Stanley Vance, Kent University: H. T. Miner professor of business administration, University of Oregon.

Otto von Fieandt: instructor in economics, Yale University.

H. Jean Waldrop: instructor in economics, Wellesley College.

Vivian C. Walsh: associate professor of economics, School of Business Administration, University of Buffalo.

Rhea H. West, Jr., Wake Forest College: associate professor of economics, College of Business Administration, University of Georgia.

Robert O. Wheeler: instructor in economics, Montana State University.

C. Arthur Williams: visiting professor of insurance, Wharton School, University of Pennsylvania.

Robert J. Wolfson, Michigan State University: appointment at University of California, Los Angeles.

Arnold Zellner, formerly University of Washington: associate professor of economics, University of Wisconsin.

Leaves for Special Appointments and Assignments

Wallace N. Atherton, Michigan State University: director, Center for Studies on Economic Development, University of the Andes, Bogotá, Colombia, 1960-61.

Emile Benoit, Columbia University: Brookings Institution professorship 1960-61.

Max R. Bloom, Syracuse University: Fulbright lecturer, Technion-Israel Institute of Technology, 1960-61.

Russell P. Bowers, De Paul University, Chicago: University of Wisconsin-Ford Foundation project, Gadjah Mada University, Jogjakarta, Indonesia.

Karl A. Fox, Iowa State University (Ames): visiting professor of economics, Harvard University, 1960-61.

Henry W. Grayson, University of Maryland: professor of economics and head of economics department, University of Khartoum, Sudan.

Frank A. Hanna, Duke University: with United Nations as industrial statistics expert, National Statistical Services of Greece, Athens, 1960-61.

Earl O. Heady, Iowa State University (Ames): Center for Advanced Study in the Behavioral Sciences, Stanford, California.

James R. Hoath, formerly Kansas State University: University of Wisconsin-Ford Foundation project at Gadjah Mada University, Jogjakarta, Indonesia.

Patrick R. Huntley, University of Arizona: U. S. Bureau of the Census, Washington, D.C.

Alexandre Kafka, University of Virginia: United Nations secretariat, fall term 1960-61.

Michael Kaser, U.N. Economic Commission for Europe: research fellow, St. Antony's College, Oxford University, 1960-61.

B. S. Keirstead, University of Toronto: visiting professor, University College of the West Indies, 1960-61.

George Kleiner, University of Illinois: International Cooperation Administration, India, for two years.

Tjalling Koopmans, Yale University: Frank W. Taussig research professor of economics, Harvard University, 1960-61.

Abba Lerner, Michigan State University: Center for Advanced Study in the Behavioral Sciences, Stanford, California.

R. W. Lindholm, University of Oregon: in Korea for review of Economic Development Program of the Republic of Korea, summer 1960, request of International Cooperation Administration.

Bruce R. Morris, University of Massachusetts: University of Wisconsin-Ford Foundation project at Gadjah Mada University, Jogjakarta, Indonesia.

Howard W. Nicholson, Clark University: Transportation Study Group, Senate Committee on Interstate and Foreign Commerce, first semester 1960-61.

Forrest R. Pitts, University of Oregon: member, International Cooperation Administration advisory group to the Economic Development Council in Korea.

Edwin P. Reubens, The City College, New York: University College of West Indies, fall semester 1960.

Alek A. Rozental: tax adviser with the International Cooperation Administration in Laos.

Philip Sheinwold, Brooklyn College; Fulbright lecturer, Quito, Ecuador.

Paul B. Simpson, University of Oregon: member, International Cooperation Administration advisory group to the Economic Development Council in Korea.

Gerhard Tintner, Iowa State University (Ames): technical consultant, United Nations Technical Assistance Board, Indian Statistical Institute, Calcutta, India, summer 1960.

Rutledge Vining, University of Virginia: visiting professor of economics, Giannini Foundation, University of California, Berkeley, fall term 1960-61.

Resignations

Edward C. Acheson: George Washington University.

Edward C. Atwood, Jr.: Washington and Lee University.

Jack Hirshleifer: Graduate School of Business, University of Chicago.

John J. Klein: Oklahoma State University.

Miscellaneous

Agustín Benítez Heymann: admitted to partnership in public accounting firm Manrara y Pérez Daple, Havana, Cuba.

FIFTY-SEVENTH LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1960, and theses undertaken in the same period.

General Economics; Methodology

Thesis in Preparation

JOAN E. ALLEN, B.S. Maryland 1958; M.A. Virginia 1959. Humanitarianism in classical economics. *Virginia.*

Price and Allocation Theory; Income and Employment Theory; History of Economic Thought

Degrees Conferred

ROBERT S. ADDEN, Ph.D. North Carolina 1960. The economic effects of governmental fiscal policy as viewed by certain English classical economists.

YOON B. AWH, Ph.D. Florida 1960. A study in the concept of balanced development with particular reference to the Nurksian doctrine of balanced growth.

IRENE H. BUTTER, Ph.D. Duke 1959. Academic economics in Holland, 1800-1870.

SAMUEL B. CHASE III, Ph.D. California (Berkeley) 1960. The theory of asset prices.

LESLIE D. FIXLER, Ph.D. New York 1960. The economics of price discrimination.

FREDERICK M. GOTTHEIL, Ph.D. Duke 1959. The economic predictions of Karl Marx: an examination of Marxian economic theory.

LAFAYETTE G. HARTER, JR., Ph.D. Stanford 1960. John R. Commons, institutional economist.

EUNICE KITCHELL, Ph.D. Texas 1959. Statistical investigation in the elasticities of demand for motor fuel.

ARNOLD B. LARSON, Ph.D. Stanford 1960. Evidence on the temporal distribution of price effects of new market information.

RONAN G. MACDONALD, Ph.D. Wisconsin 1960. A comparison of the theories of entrepreneurial expectations of Keynes and Schumpeter.

SANFORD L. MARGOSHES, Ph.D. New York 1960. Economic theory and entrepreneurial techniques of profit measurement.

BERNARD J. MARKS, Ph.D. Minnesota 1960. The prediction and analysis of demand for replacement parts.

BENTON F. MASSELL, Ph.D. Yale 1960. Determinants of productivity change in United States manufacturing.

JOHN W. NEVILE, Ph.D. California (Berkeley) 1960. Investment theory in some modern dynamic economic models.

JOHN C. S. PARK, Ph.D. Nebraska 1959. Value theory and oligopolistic manufacturing industries.

HERBERT R. RUNYON, Ph.D. Michigan 1960. Contributions of Irving Fisher to modern theory.

KAZUO SATO, Ph.D. Yale 1960. Price-cost structure and behavior of profit margins in manufacturing.

JOHN M. SCHEIDELL, Ph.D. Notre Dame 1960. The time elasticity of demand and productivity: a theoretical analysis.

CHARLES L. SCHULTZE, Ph.D. Maryland 1960. Recent inflation in the United States.

HOWARD D. SHARPE, JR., Ph.D. Harvard 1960. The effects of secular growth in factor

money income rates, productivity, and labor force on the secular level of, and growth of, employment.

HENRY SOLOMON, Ph.D. New York 1960. The significance of the concept of capacity to produce in economic theory.

TADIMI TACHINO, Ph.D. American 1960. Aspects of the theory of production and production functions.

JOHN W. L. WINDER, Ph.D. Chicago 1960. The demand for stocks: copper.

TENG-PIN YU, Ph.D. New York 1960. Variables of an aggregate consumption function.

Theses in Preparation

DONALD V. BEAR, B.A. Princeton 1954; B. Phil. Oxford 1956. The nature of prices in macro-dynamic economic processes. *Stanford.*

GARY BICKEL, B.A. Colorado 1955. Factor proportions and relative prices: an experiment in estimation of shadow prices. *Stanford.*

B. D. BIXLEY (earlier degrees not supplied). Welfare problems and implications of the theory of economic growth. *Toronto.*

STANLEY BOBER, B.A. New York 1953; M.A. 1956. A study of the cyclical behavior of retail inventories. *New York.*

WILLIAM BREIT, B.A. Texas 1955; M.A. 1956. The wages-fund theory: a historical analysis and restatement. *Michigan State.*

VARTKES BROUSSALIAN, B.S. London School of Econ. 1951. Inflation-caused redistribution of wealth: a test of a hypothesis. *California (Los Angeles).*

DOUGLAS C. DACY, B.B.A. Texas 1950; M.A. 1954. A study of land prices in the United States. *Harvard.*

MARY HAMILTON (earlier degrees not supplied). Empirical implications of demand theory. *Pennsylvania.*

JOHN HENNING, B.S. Columbia 1955; M.A. Yale 1956. Trends in price flexibility: 1923-1959. *Cornell.*

NORMAN S. HUBBARD, B.A. Yale 1956; M.A. 1957. Cost reduction and the inducement to invest. *Yale.*

VELY M. LEROY, B.A. College of Notre Dame 1954; M. Comm., École des Hautes Études 1957. The Cambridge equation, the real balance effect and employment. *Michigan State.*

SELIG D. LESNOY, B.A. Michigan 1948; M.S. Columbia 1953. The cost of capital in the theory of investment. *Michigan.*

ABDULHADI MADJID, B.A. Harvard 1952. Flow of Funds. *Harvard.*

F. MARTIN, B.A. McGill 1952; M.A. 1958. Location theory. *McGill.*

WILLIAM M. PARKER, B.A. California (Berkeley) 1938; M.A. 1948. The use of accounting data to test and integrate theories of the firm. *Southern California.*

FRANK PETRELLA, JR., B.A. Notre Dame 1956; M.A. 1957. Edmund Burke as a conservative classical economist. *Notre Dame.*

WOLFGANG SCHOELLKOPF, B.A. California 1956. A microeconomic approach to consumption theory. *Cornell.*

KRISHNA PRASAD SHARMA, B.A. Calcutta 1951; M.A. 1954. Studies in some growth models with special reference to the U. S. economy. *Oregon.*

PHILIP E. SORENSEN, B.S. Utah State Agricultural 1954; M.S. 1957. The economics of Francis Ysidro Edgeworth. *California (Berkeley).*

THIRUKODIKAVAL NILAKANTA SRINIVASAN, B.A. Madras 1953; M.A. Indian Statistical Inst. 1955; M.A. Yale 1958. Investment criteria and choice of techniques of production. *Yale.*

DOMENICO A. TOSATO, B.A. Bowdoin 1956; M.A. Yale 1958. Uncertainty and the implementation of macroeconomic policy. *Yale.*

IAN D. S. WARD, B.C., M.C. Melbourne. Productive and unproductive consumption in pre-classical and classical literature. *California (Berkeley).*

SIDNEY G. WINTER, JR., B.A. Swarthmore 1956; M.A. Yale 1957. Relations between research spending and corporate growth: a theoretical and statistical analysis. *Yale.*

DE-MIN WU, B.A. National Taiwan Univ. 1956. A decision-unit model of household expenditures on durable goods. *Wisconsin.*

Economic History; Economic Development; National Economies

Degrees Conferred

LORENZO M. BELOTTI, Ph.D. Texas 1960. The influence of Keynesian "Dirigisme" on Christian socialism in the postwar Italian economic policies.

JAMES H. BLACKMAN, Ph.D. Columbia 1960. Soviet transport and the process of industrialization.

BERNARD G. BROWN, Ph.D. Wisconsin 1959. The fiscal influence on economic development, with special reference to Iran.

EDWIN CALDWELL, Ph.D. Harvard 1960. The development of manufacturing in Texas since 1919.

YU-MIN CHOU, Ph.D. Illinois 1960. The role of international trade in the economic development of Southeast Asia, with particular reference to Malaya and Indonesia.

EDWARD F. CRIM, JR., Ph.D. Illinois 1960. The effect of regional public expenditures on the level of regional income as illustrated by the State of Oklahoma.

PADMA R. DESAI, Ph.D. Harvard 1960. A short term planning model for the Indian economy.

GUIDO J. M. DI TELLA, Ph.D. Mass. Inst. of Tech. 1960. Economic history of Argentina: 1914-1933.

JOHN M. FRIKART, Ph.D. Colorado 1959. Effects of the Peron regime on the Argentine economy.

GEOFFREY B. HAINSWORTH, Ph.D. California (Berkeley) 1960. Classical theories of overseas development.

JOSEPH M. HENNESSEY, Ph.D. Boston College 1960. New England manufacturing, 1947-1954; a postwar analysis of employment, output and productivity.

JOHANNES HIRSCHMEIER, Ph.D. Harvard 1960. The genesis of modern entrepreneurs in Meiji, Japan.

MOON H. KANG, Ph.D. Nebraska 1960. The monetary aspect of the economic development in Japan with special reference to monetary policies, 1868-1935.

EDWARD S. LITTLE, Ph.D. American 1959. National resources of Spain as a basis for an industrial economy.

PAUL MEDOW, Ph.D. Columbia 1960. Conceptual and methodological problems in applying Schumpeter's theory of economic development to nonmarket economies.

JORA MINASIAN, Ph.D. Chicago 1960. The economics of research and development.

HARRY A. MISKIMIN, Ph.D. Yale 1960. Specie debasement and price movements in France, 1295-1395.

MAX MUELLER, Ph.D. Illinois 1960. Money, investment and economic development with special reference to India.

HANEEF A. NASEEM, Ph.D. American 1960. Progress of economic growth in Pakistan (1947-57): a critical study in retrospect.

TOYOKI OKABAYASHI, Ph.D. Oregon 1960. Measuring the contributions of natural resources to the national outputs of the United States and Japan.

IZZID D. PAL, Ph.D. McGill 1960. Commercial policy and economic development with reference to Pakistan.

THOMAS T. POLEMAN, Ph.D. Stanford 1960. The Papaloapan development project.

ZORA PROCHAZKA, Ph.D. Harvard 1960. Foreign trade and economic development of Czechoslovakia.

BEATRICE G. REUBENS, Ph.D. Columbia 1960. State financing of private enterprise in early New York.

SAMUEL M. ROSENBLATT, Ph.D. Rutgers 1960. The House of John Norton & Sons: a study of the consignment method of marketing tobacco from Virginia to England.

ROKNEDDIN SADAT-TEHRANI, Ph.D. New York 1960. The seven year plan organization for the economic development of Iran.

JOGINDER S. SAHOTA, Ph.D. Oregon 1960. Economic criteria for determination of methods of production for Indian agriculture.

MARTIN C. SCHNITZER, Ph.D. Florida 1960. The use of inducements by states and communities in the promotion of industrial development, with special reference to Mississippi.

SAID A. SHAH, Ph.D. McGill 1960. Structural obstacles to economic development in India.

ROBERT G. SPIEGELMAN, Ph.D. Columbia 1960. The production of the Indian steel industry in the inter-war period.

HAZEL J. WALDROP, Ph.D. Southern California 1960. A study of the major theories of economic development and decline.

IVOR P. WOLD, Ph.D. Texas 1960. Economic change in Canada, pre-war to recent, emphasizing aggregates.

Theses in Preparation

JAMSHID ASHRAFI, B.A. California 1953; M.A. San Francisco State 1955. Criteria for investment decision in underdeveloped economies—with special reference to Iran. *Southern California.*

ELIEZER AYAL, M.A. Hebrew 1954. Thailand's economy and public policies under the constitutional government—a case study of an underdeveloped country. *Cornell.*

VIRBHAN G. BHATIA, B.Sc. Banara Hindu 1951; M.A. Panjab 1954; M.A. Harvard 1959. Locational approach to economic development. *Harvard.*

BRIAN W. BROGAN, B.Comm. Melbourne. Location problems in economic development. *Johns Hopkins.*

ANDRZEJ BRZESKI, M.A. Lediz (Poland). Inflation in post-war Poland. *California (Berkeley).*

YOUNGIOB CHUNG, B.S. California (Los Angeles) 1952; M.A. Columbia 1955. State and capital formation in Japan. *Columbia.*

GEORGE A. DONELY, B.A. Williams 1956; M.A. Columbia 1958. The reaction of New Orleans and the lesser Gulf ports to the Erie Canal and other East-West transportation improvements in the 19th century. *Columbia.*

DANIEL J. EDWARDS, B.A. Maryland 1956; M.A. 1958. The process of inflation in a neutral country: Sweden during World War II. *Virginia.*

LATTEE A. FAHM, B.A. California (Berkeley) 1957; M.A. 1958. Economic planning in Nigeria: 1950-1975. *Massachusetts Inst. Technology.*

A. GERLOF HOMAN, B.A. Bethel 1949; M.S. Kansas State 1952. The role of agriculture in development in Latin America. *Oregon.*

CONSTANTINA S. KALMAN, B.A. American (Cairo) 1956; M.A. Stanford 1957. The growth of the Greek economy, 1927-1959. *Johns Hopkins.*

RICHARD H. KAUFMAN, B.A. Brandeis 1957; M.A. Harvard 1959. The impact of commercial measures on the economic development of Israel. *Harvard.*

MICHAEL KAVANAGH, B.A. University College Dublin 1952; M.A. 1954. An application of growth model concepts to the programming of development in the Irish economy. *Fordham.*

DAVID J. LOSCHKY, B.A. Missouri 1956; M.A. Harvard 1958. The development of productivity in the American distribution system. *Harvard.*

K. S. MALI, B.A. Panjab 1941; M.A. 1944. Financing economic development of Burma since Independence. *Indiana.*

CHARLES Y. MANSFIELD, B.A. Oberlin 1955; M.P.A. Princeton 1958. Development banks: their role in economic growth. *Princeton.*

PLACIDO L. MAPA, B.A. Ateneo de Manila 1955; M.A. St. Louis 1957. Development financing in an underdeveloped country. *Harvard.*

ROBERT MUSCAT, B.A. Columbia 1952; M.A. 1957. Foreign aid and economic development. *Columbia.*

RAMPRASAD B. PANDIT, B.A. Ahmedabad 1948; M.B.A. Southern California 1960; M.A. 1960. Planning techniques and economic development: India and China, 1945-59. *Southern California.*

ABHAYKUMAR C. PARIKH, B.A. Gujacat 1952; M.A. Bombay 1955. India banking and economic development since 1947. *American.*

HOLLIS W. PETER, B.S. Wisconsin 1938; M.S. Michigan 1956. Technical knowledge and economic growth: a case study of the Philippines. *Michigan.*

ABDUL QADIR, B.A. Delhi College of Commerce 1949; M.A. Karachi 1953. Take-off to economic growth: a case study of Pakistan. *Clark.*

LAURA RANDALL, M.A. Massachusetts 1959. The effects of large supplies of agricultural labor on Mexican economic development—1940 to date. *Columbia.*

A. ROTSTEIN (earlier degrees not supplied). Institutional foundations of Canadian industrial development. *Toronto.*

HOWARD F. SMITH, B.A. Wayne 1940; M.B.A. Harvard 1942. An analysis of Ceylon's ten year plan of economic development. *American.*

SNOH UNAKUL, B.Comm. Melbourne 1954; M.A. Columbia 1958. Formulation and economic appraisal of Thailand's public development projects. *Columbia.*

JAMES R. WASON, B.A. American 1949. Labor in the federal city in the Jacksonian era. *American.*

JOHN A. WEIR, B.S. St. Dunstan's, P.E.I. 1953; M.B.A. Univ. Western Ontario 1955. Rural reconstruction on Prince Edward Island: an evaluation. *Notre Dame.*

GEORGE R. WINTER, B.S. Alberta 1955; M.S. Iowa (Ames) 1958. External economies in relation to underdeveloped areas. *Iowa (Ames).*

LUIS F. YEPEZ, B.S. Wisconsin 1959; M.S. Wisconsin 1959. Market structure considerations and the theory of economic development. *Wisconsin.*

JUNE ZACCONE, M.A. North Carolina 1954. Comparative economic development of India and China. *North Carolina.*

Statistical Methods; Econometrics; Social Accounting

Degrees Conferred

JOHN O. BLACKBURN, Ph.D. Florida 1959. A balance sheet for the nation: a study in concepts.

FRANKLIN M. FISHER, Ph.D. Harvard 1960. A priori information and time series analysis.

SEYMOUR GOODMAN, Ph.D. Johns Hopkins 1960. Patterns of income inequality in states

JOHN F. HABERER, Ph.D. Duke 1960. Some conceptual problems in moneyflows accounting: United States and Canada.

PAUL B. HENDERSON, JR., Ph.D. Mass. Inst. of Tech. 1960. Theory of data systems for economic decisions.

JANET M. HOOKS, Ph.D. Illinois 1960. The contribution of women to national income.

RUBIN SAPOSNIK, Ph.D. Minnesota 1959. Models for the analysis of capital equipment purchase policies.

Theses in Preparation

DAVE BRAMHALL (earlier degrees not supplied). Social accounting for regions. *Pennsylvania.*

JAMES MCKENNEY, B.S. Purdue 1952; M.S. 1952. A conceptual framework for multiprogramming activities. *California (Los Angeles).*

NORMAN RUSHFORTH, B.Sc. Birmingham 1954. A procedure for testing the hypothesis that K sample correlation matrices are from the same population. *Cornell.*

Economic Systems; Planning and Reform; Cooperation

Degrees Conferred

JOHN S. HOYT, JR., Ph.D. American 1959. An investigation of the economics of Soviet locational doctrine, policy and practice: with special emphasis on heavy industry.

MOHAMMED IMADY, Ph.D. New York 1960. Economic programming in the Syrian region of the United Arab Republic.

Theses in Preparation

FUAD S. ABU-ZAYYAD, B.A. Berea 1957; M.A. Fletcher School 1958; M.A.L.D. 1959. Economic implications of Arab unity. *Fletcher School.*

MAGDI M. EL KAMMASH, B.Comm. Cairo 1952; D.A. 1956; M.P.H. North Carolina 1958. On the use of national accounting in planning models for underdeveloped countries. *Duke.*

HY SANG LEE, B.A. Elmhurst 1958; M.S. Wisconsin. Economic planning in India and Communist China: a comparison. *Wisconsin.*

DAVID MERMELSTEIN, B.A. Amherst 1955. Recent theories on American capitalism. *Columbia.*

RAYMOND J. MONSEN, JR., B.S. Utah 1953; M.A. Stanford 1954. Ideologies of modern American capitalism. *California (Berkeley).*

Business Fluctuations

Degrees Conferred

YOSSEF ATTIYEH, Ph.D. Chicago 1959. Wage-price spiral versus demand inflation: United States, 1949-1957.

PETER D. STERNLIGHT, Ph.D. Harvard 1960. United States credit policy in the 1954-57 period.

RONALD P. WILLETT, D.B.A. Indiana 1959. A model for forecasting economic activity in Indiana and its subregions based on the use of current economic indicators.

Theses in Preparation

VINCENT CANGELOSI, B.S. Louisiana State 1954; M.B.A. 1956. Forecasting methods for predicting gasoline consumption. *Arkansas.*

VICTOR GARLIN, B.A. California (Berkeley) 1956. Technological change and business cycles in the post-war period. *California (Berkeley).*

CLARENCE L. HAM, B.S. Wisconsin 1947. The recession of 1957-58. *California (Berkeley).*

MICHEL E. A. HERVÉ, Diplome Hautes Études Commerciales 1950; M.A. Harvard 1955. French inflation 1952-1958. *Harvard.*

JOHN H. NIEDERCORN, B.A. Yale 1956; M.A. Harvard 1959. An econometric study of inflation. *Harvard.*

JOHN P. ONDRECHEN (earlier degrees not supplied). Business cycles in the period after World War II. *Pennsylvania.*

RUDOLPH G. PENNER, B.Comm. Toronto 1958. International influences on the effectiveness of stabilization policy in Canada. *Johns Hopkins.*

JACK RICHARDSON, B.Comm. Toronto 1957. International transmission of inflation: the Canadian case. *Johns Hopkins.*

HENRY L. WOJTYLA, M.A. Chicago 1953. Cyclical interrelationships in the postwar period. *Chicago.*

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Degrees Conferred

ABDELHAK BELKORA, Ph.D. Colorado 1960. Monetary policy in two post-accord recessions, 1953-54, 1957-58.

WILLIAM BERANEK, Ph.D. California (Los Angeles) 1959. A study on the cost of capital.

ORESTES W. CANDILIS, Ph.D. Georgetown 1960. Monetary and credit policies of the Bank of Greece and their influence on the Greek post-war economy, 1944-1958.

ROBERT R. EDMINSTER, Ph.D. California (Berkeley) 1960. Money and credit in Mexico, 1920-1940.

RACHEL FLOERSHEIM, Ph.D. Johns Hopkins 1960. Financial intermediaries in Israel, 1950-1954.

PETER G. FOUSEK, Ph.D. Columbia 1960. Foreign central banking: the instruments of monetary policy.

RICHARD W. GRAVES, D.B.A. Indiana 1960. Anti-inflationary techniques of selected European countries.

HARRY P. GUENTHER, D.B.A. Indiana 1959. Commercial bank lending and investing behavior during a period of restrictive federal monetary policy.

T. E. HOLLANDER, Ph.D. Pittsburgh 1960. Economic significance of measures of capital formation and capital consumption in the business sector of the economy.

MARSHALL KAPLAN, Ph.D. Chicago 1960. Neoclassical monetary theory.

ALBERT M. LEVENSON, Ph.D. Columbia 1960. Differentials in interest rates and the cost of commercial bank lending.

DAVID B. MCCALMONT, Ph.D. Johns Hopkins 1960. Redistribution of gold reserves among Federal Reserve banks.

DANILO ORESCANIN, D.B.A. Indiana 1960. Organization of savings and loan associations.

HUGH T. PATRICK, Ph.D. Michigan 1960. The Bank of Japan: a case study in the effectiveness of central bank techniques of monetary control.

JAMES P. QUIRK, Ph.D. Minnesota 1959. Default risk and the loan market.

KARL W. ROSKAMP, Ph.D. Michigan 1960. Economic growth, capital formation, and public policy in West Germany, 1948-57.

HIRENDRA N. ROY, Ph.D. Stanford 1960. The role of monetary policy in economic development: a study of the activities of the Reserve Bank of India, 1949-56.

DONALD H. SAUER, D.B.A. Indiana 1959. The supply of and the demand for nonfarm residential mortgage funds, 1960-70.

VINODCHANDRA C. SHAH, Ph.D. Columbia 1960. Monetary policy and economic development with reference to India.

CASE M. SPRENKLE, Ph.D. Yale 1960. Warrant prices as indicators of expectations and preferences.

RICHARD H. TIMBERLAKE, JR., Ph.D. Chicago 1959. Treasury monetary policies from Jackson to Lincoln.

ROY E. TUTTLE, Ph.D. Minnesota 1959. Leaseholds—their financial consequences and disclosure.

DONALD A. TYREE, Ph.D. Texas 1959. The small-loan industry in Texas.

TALHA YAFFI, Ph.D. Wisconsin 1959. The monetary and banking system of Lebanon, with special reference to monetary reform.

Theses in Preparation

DANIEL AHEARN, B.A. Columbia 1949. Aspects of Federal Reserve policy, 1951 to date. *Columbia.*

CHARLES C. BAKER, JR., B.A. Duke 1955; M.A. Harvard 1958. Commercial bank policy and participation in regional growth. *Harvard.*

ARTHUR BENAVIE, B.A. Wayne 1954; M.A. Michigan 1955. A statistical analysis of the impact of the behavior of non-bank financial institutions on the effectiveness of monetary policy. *Michigan.*

NORMAND R. V. BERNARD, B.A. Assumption 1955; M.A. Boston College 1957. The "Bills Only" technique of open market operations. *Boston College.*

EUGENE A. BRADY, B.A. Washington 1952; M.A. 1954. A study in mortgage credit and monetary policy. *California (Berkeley).*

WILLIS K. BRAMWELL, JR., B.S. Arizona 1950; M.A. Columbia 1955. Commercial bank holdings of consumer credit and central bank monetary policy. *Columbia.*

ALBERT BRETON, B.S. St. Boniface. The demand for money: recent Canadian experience. *Columbia.*

CONRAD P. CALIGARIS, B.B.A. Clark 1955; M.A. Brown 1958. A multiple correlation analysis of factors affecting net earnings of member banks in the First Federal Reserve District. *Brown.*

ORESTES W. CANDILIS (earlier degrees not supplied). Monetary and credit policies of the Bank of Greece and their influences on the Greek post-war economy, 1944-1958. *Georgetown.*

KANG CHAO, B.A. Taiwan 1951; M.A. Michigan 1957. Demand for cash by manufacturing corporations in the U. S. *Michigan.*

HAROLD L. CHEADLE, B.A. Miami (Ohio) 1940. A re-examination of monetary velocity analysis. *American.*

HENRY J. CLAYCAMP, B.A. Washburn 1956; M.A. Illinois 1957. Structure of consumer savings. *Illinois.*

ADEL ELGOWHARY, B.Comm. Cairo 1951. Financial structure and monetary policy of Egypt. *Syracuse.*

ALBERT FISHLOW, B.A. Pennsylvania 1956. A study in American monetary history. *Harvard.*

RICHARD FRIEDMAN, B.A. City College of New York 1957. Open-market operations and liquidity theory. *Johns Hopkins.*

DONALD D. HESTER, B.A. Yale 1957; M.A. 1958. An empirical examination of a loan offer function by commercial banks. *Yale.*

RAYMOND W. HOOKER, B.S. Missouri 1956; M.S. 1959. Bank financing of business. *Wisconsin.*

HAROLD L. JACKSON, B.A. California (Berkeley) 1956. Development of financial institutions, 1919-1933. *California (Berkeley).*

ROBERT A. JOHNSTON, B.Comm. Toronto 1956. An analysis of the Canadian floating discount rate and its implications for monetary policy. *Yale.*

BERISLAV KARCIC, B.A. Columbia 1955. Monetary policy of Yugoslavia, 1948-1960. *Columbia.*

DIONYSSIOS S. KOTSONIS, B.A. Bowdoin 1953. The devaluation of the Greek drachma, 1954. *Columbia.*

LEONARD LAUDADIO, B.A. College of Puget Sound 1956; M.A. Washington 1957. The adequacy of bank earnings. *Washington.*

EARL MARTINSON, B.A. San Diego State 1951; M.B.A. California (Los Angeles) 1952. Management organization in the savings and loan industry. *California (Los Angeles).*

BRUCE T. MCKIM, B.S.C. Iowa 1950; M.A. 1958. Monetary velocity in the United States; from the Accord through 1959. *Iowa.*

JOHN N. MCKINNEY, B.A. Washington 1954. Some aspects of the term structure of interest rates in the United States. *California (Berkeley).*

JACOB P. MEERMAN, B.A. Chicago 1954; M.A. 1958. Nicholas Biddle on central banking. *Chicago.*

NICOCLES MICHAS, B.A. City College of New York 1957. Qualitative credit control in underdeveloped countries. *Columbia.*

GAIL E. MULLIN, B.A. Wabash 1952; M.B.A. Indiana 1953. Growth and structural change in the savings and loan business. *Indiana.*

KOJI NAKAGAWA, B.A. Kyoto Imperial Univ. 1953; M.A. Syracuse 1960. A money-flow approach to the analysis of cost-push inflation. *Syracuse.*

CHARLES F. PEAKE, B.S. East Tennessee State 1956; M.S. Tennessee 1958. Financial intermediaries in the United Kingdom, 1850-1960. *Maryland.*

BENEDICT J. PEDROTTI, B.A. Michigan 1952; M.A. 1956. Commercial bank portfolio adjustments during the period 1955-57. *Michigan.*

R. BRUCE RICKS, B.S. California 1956; M.B.A. 1957. Trend to equity real estate investment by institutions in the post-war period. *California (Berkeley).*

BYONG HYUN SHIN, M.A. American 1954. Monetary and fiscal policy in South Korea, 1954-1960. *Columbia.*

EDWARD P. SNYDER, B.A. Oberlin 1948. The homogeneity of the member bank business. *Chicago.*

JAMES E. SUTTON, B.S. Wisconsin 1955. The composition of liquid asset holdings. *Michigan.*

JOSE VERGARA, B.A. Maryland 1956. An analysis of opposition to the quantity theory of money. *Virginia.*

NORMAN E. WEIR, B.A. Oklahoma 1935; M.A. Colorado 1941. Consumer instalment credit as a variable in the quantity and velocity of money in California, 1952-1960. *Southern California.*

HARVEY J. WHEELER, B.A. Maine 1957. An examination of the hypothesis that money is a "luxury" good. *Virginia.*

Public Finance; Fiscal Policy

Degrees Conferred

SAMUEL BLITMAN, Ph.D. Columbia 1960. The invested capital standard in the United States excess profits tax.

JOHN D. COUPE, Ph.D. Clark 1960. Municipal debt control in Massachusetts and its countercyclical implications.

IRVING GOFFMAN, Ph.D. Duke 1959. Erosion of the personal income tax base in Canada and the United States.

JOHN F. GRAHAM, Ph.D. Columbia 1960. Provincial-municipal fiscal relations and economic development in a low-income province: Nova Scotia.

WALTER T. GREANEY, JR., Ph.D. Harvard 1960. Expenditures and the budget in Massachusetts.

JOSEPH R. GUERIN, Ph.D. Pennsylvania 1960. The development of the theory of excise taxes.

REED R. HANSEN, Ph.D. Wisconsin 1960. The tax treatment of family income.

JAMES M. HEIDELL, Ph.D. New York 1960. The purchasing of tax-exempt bonds by individuals in the 1946-1956 decade.

ROBERT KINSEY, Ph.D. Columbia 1960. The role of lotteries in public finance.

HARRY H. LANDRETH, JR., Ph.D. Harvard 1960. The measurement of local fiscal capacity.

JAMES H. MALOON, Ph.D. Indiana 1960. The Ohio death taxes.

HAROLD F. MCCLELLAND, Ph.D. Harvard 1960. Tax aspects of the variable annuity.

WILLIAM J. MCKINSTRY, Ph.D. Yale 1960. An estimate of the impact of increasing death tax rates on beneficiaries of federally taxable estates.

ALEXANDER S. POW, Ph.D. New York 1960. The comptroller general and the General Accounting Office of the United States.

ARNOLD H. RAPHAELSON, Ph.D. Clark 1960. Massachusetts unemployment compensation 1948-57; a study in countercyclical finance.

CHUNG-HIEH (JOHN) RIEW, Ph.D. Wisconsin 1960. Forty years of property values in Wisconsin.

RODERICK H. RILEY, Ph.D. Wisconsin 1959. The "bonding period" in federal taxation of distilled spirits.

ROBERT L. SLIGHTON, Ph.D. Johns Hopkins 1960. Taxation and investment incentives.

EDWARD K. SMITH, Ph.D. Harvard 1960. The Massachusetts income tax.

JOHN D. STRASMA, Ph.D. Harvard 1960. State and local taxes paid by manufacturers: a new comparison.

JOSHUA WACHTEL, Ph.D. New York 1959. The effect of litigated cases on the Internal Revenue Code.

LAURENCE N. WOODWORTH, Ph.D. New York 1960. Taxation by the United States of income earned abroad.

GEORGE S. ZARKOS, Ph.D. Indiana 1960. The sales tax system of Greece.

Theses in Preparation

JOHN R. ALLAN, B.A. McMaster 1955. The effect of fiscal policy in redistributing incomes in Canada. *Princeton.*

ROBIN BARLOW, B.A. Oxford 1954; M.B.A. Michigan 1958. The role of fiscal policy in the economic development of Egypt. *Michigan.*

EARL S. BEECHER, B.A. Utah 1949; M.B.A. California (Los Angeles) 1956. Public finance in California. *California (Los Angeles).*

RICHARD M. BIRD, B.A. King's College (Canada) 1958. Initial allowances and investment allowances under the British income tax: countercycle policy. *Columbia.*

ROBERT O. BOSTON, B.S. Alabama 1949; M.S. 1950. An economic analysis of the problem of controlling municipal indebtedness in Alabama. *Alabama.*

EDITHA BRANNOCK, A program for equitable financing of the public welfare function in North Carolina. *North Carolina.*

EDWARD W. BRENNAN (earlier degrees not supplied). The personal property tax in Pennsylvania. *Pennsylvania.*

ELIZABETH J. DAVID, B.A. Swarthmore 1953; M.A. Michigan 1957. The economic implications of attitudes toward state and local finance. *Michigan.*

ARAKKAL T. EAPEN, B.A. Madras 1945; M.B.A. Michigan 1954. A study of federal finance in selected countries: U.S., Australia, Canada, and India. *Michigan.*

KARL D. GREGORY, B.A. Wayne State 1952; M.A. 1957. State and local expenditures for publicly supported institutions of higher education. *Michigan.*

JAMES HEILBRUN, B.S. Harvard 1945; M.A. 1947. Real estate taxation and rehabilitation of urban dwellings. *Columbia.*

LEON KOROBOW, B.A. Brooklyn 1954. Relevance of theory of risk to public expenditures for life-saving and accident-averting purposes. *Columbia.*

WILLIAM E. LAIRD, B.S. Stetson 1956; M.A. George Washington 1958. The theory of debt management. *Virginia.*

ABU N. M. MAHMOOD, M.A. Dacca 1942. Deficit-financing for economic development-case study. *Harvard.*

ERLING O. NAESETH, B.A. Luther 1947; M.S. Wisconsin 1949. Financial support for education in Iowa. *Wisconsin.*

ABDEL RAHMAN, B.A. Cairo 1949; M.A. Kentucky 1957. The Egyptian income taxation of non-resident alien and foreign corporations and its effect on the Egyptian economic growth. *Indiana.*

ROBERT W. RESEK, B.S. Illinois 1957; M.A. Harvard 1960. Fiscal policy and economic growth. *Harvard.*

G. REZEK, B. of Bus. Admin. American Univ. (Beirut) 1951; M.A. McGill. Public finance with special reference to underdeveloped countries of the Middle East. *McGill.*

MICHAEL D. TANZER, B.A. Harvard 1957; M.A. 1960. The income elasticity of state and local governmental revenues and expenditures. *Harvard.*

ARAN THAMMANO, B.A. Thammasat (Thailand) 1953; M.A. Michigan 1958. An evaluation of public debt management policies, 1946-1958. *Oregon.*

JAN V. TUMLIR, B.A. Yale 1953; M.A. 1955. Taxes, public expenditures and the balance of payments: Germany, 1954-58. *Yale.*

LYLE L. TYNER, B.S. Kansas 1933; B.A. Denver 1948. Special assessments in local government finance. *Wisconsin.*

International Economics

Degrees Conferred

DAVID J. ASHTON, Ph.D. Fletcher School 1959. The meaning of export origin.

ROBERT F. BARLOW, Ph.D. Fletcher School 1960. United States exports and imports of cotton textiles.

NATHAN BRODSKY, Ph.D. American 1959. Devaluation and the British dollar gap in the postwar period 1946-55.

ARCHIBALD C. CALLAWAY, Ph.D. Harvard 1960. Adjustment problems of a primary exporting economy.

MANUEL O. DIAZ, Ph.D. Pennsylvania 1960. The Spanish average principle as practiced in the trade between Spain and the Indies.

ROBERT R. DINCE, Ph.D. Cornell 1960. The lending policies of the Export-Import Bank, 1945-52.

LYMAN A. DREWRY, JR., Ph.D. Virginia 1960. Offsetting interventions in the international market: cotton and cotton textiles as a case study.

HORST H. H. ESCHENBERG, Ph.D. Purdue 1960. German balance of payments problems since 1950.

RONALD E. FINDLAY, Ph.D. Mass. Inst. of Tech. 1960. Essays on some theoretical aspects of economic growth.

RICHARD L. GORDON, Ph.D. Mass. Inst. of Tech. 1960. Coal pricing and the energy problem in the European community.

JACK D. GUENTHER, Ph.D. Harvard 1960. The Mexican balance of payments, 1950-58.

CHARLES W. HULTMAN, Ph.D. Iowa 1960. Agricultural surplus disposal and foreign aid.

STEPHEN H. HYMER, Ph.D. Mass. Inst. of Tech. 1960. International operations of national firms—a study of direct foreign investment.

LELAND LANGBEIN, Ph.D. Pittsburgh 1960. International movement of petroleum and petroleum products.

JUNG-CHAO LIU, Ph.D. Michigan 1960. An econometric model of the rice market in the Japanese Empire, 1910-1937.

SARAH S. MONTGOMERY, Ph.D. Wisconsin 1960. The terms of trade of primary products and manufactured goods in international trade, 1872-1952.

ROBERT S. OZAKI, Ph.D. Harvard 1960. Japan's postwar resurgence in international trade.

MARCEL K. RICHTER, Ph.D. Mass. Inst. of Tech. 1959. Some economic problems in an uncertain world.

HERBERT I. SCHILLER, Ph.D. New York 1960. The United States Congress and the American financial contribution to the United Nations Relief and Rehabilitation Administration.

ROBERT G. SCHROEDER, Ph.D. California (Berkeley) 1959. British and the European common market.

MARTIN W. WILMINGTON, Ph.D. New York 1960. Economic regionalism in the Middle East during World War II (the Middle East supply center).

Theses in Preparation

ROBERT J. BALL (earlier degrees not supplied). Econometric analysis of the U. K. balance of payments. *Pennsylvania.*

VLADIMIR N. BANDERA, B.A. Connecticut 1954. International capital movements in Eastern Europe between the World Wars. *California (Berkeley).*

RUTH A. BIRDZELL, M.S. Illinois 1952. Postwar investment in Australia. *Illinois.*

DAVID W. BODENBERG, B.A. Yale 1955; M.A. Princeton 1958. The soft loan: studies in international trade theory and contemporary policy. *Princeton.*

GEORGE H. BOSSY, M.A. Columbia 1956. Effects of commercial policy on economic development in Australia. *Columbia.*

RICHARD N. COOPER, B.A. Oberlin 1956; M.Sc.Econ. London 1958. Inflation, growth and the U.S. balance of payments, 1956-59. *Harvard.*

ANA N. EAPEN, B.A. Univ. of the Philippines 1950; M.A. Michigan 1958. Exchange controls in the Philippines. *Michigan.*

A. GEORGE GOLS, B.A. Upsala 1954; M.A. Johns Hopkins 1957. United States foreign petroleum investments and public economic policy. *Oregon.*

WARREN R. HARDEN, B.A. Iowa State Teachers 1950; M.A. Colorado 1951. Central American economic integration. *Indiana.*

MOHAMMED P. HASAN, B.A. Panjab 1950; M.A. 1952. Inflation and balance of payments: the experience of Pakistan, 1950-59. *Yale.*

CHARLES W. HULTMAN, B.A. Upper Iowa 1952; M.A. Drake 1957. Agricultural surplus disposal and foreign aid. *Iowa.*

OMESH KHANNA, B.A. Albion 1953; M.B.A. Michigan 1955. The balance of payments difficulties in semi-planned economies: the Indian experience. *Michigan.*

HEINZ KOHLER, Vordiplom Free Univ. of Berlin 1956; M.A. Michigan 1958. International economic relations within the Communist bloc. *Michigan.*

HERMES LEMONIAS, B.A. American Univ. (Cairo) 1954; M.A. Brown 1956. European economic integration and Greece. *Brown.*

ROBERT LIPSEY, B.A. Columbia 1944. Study of trends in U.S. international trade: export and import volume and price indexes for U.S. foreign trade since 19th century. *Columbia.*

RICHARD D. MALLON, B.A. Princeton 1949. Industrialization and import replacement. *Harvard.*

JOSEPH A. MARTELLARO, B.A. Notre Dame 1956; M.A. 1958. World bank loans to Italy and the effects on post-war economic growth. *Notre Dame.*

MICHAEL MISCHAIKOW, M.A. Indiana 1957. Postwar effects of variations in German fuel requirements upon United States coal exports. *Indiana.*

RUSSELL MORAN, B.A. California 1954. Trade nationalization in the Philippines: an instrument of national economic policy. *Cornell.*

GORDON R. MUNRO, B.A. British Columbia 1956; M.A. Harvard 1959. Evolution of the British foreign exchange control system in the postwar era. *Harvard.*

AHMAD A. MURAD, B.A. Washington State 1956; M.A. Wisconsin 1960. Problems of capital accumulation in Egypt. *Wisconsin.*

JAMES M. MURRAY, B.S. North Dakota 1956; M.A. 1958. Taxation and private foreign investment. *Oregon.*

SAID NABULSI (earlier degrees not supplied). The theory and the practice of economic integration as applied to the monetary aspects of the Syrian-Egyptian union. *Georgetown.*

JAMES K. NETTLES, B.S.C. Spring Hill 1957. The interregional balance of payments of the United States. *California (Berkeley).*

JOSEPH G. POLACH, LL.D. Masaryk (Czechoslovakia). Euratom: a study in the European economic integration. *American.*

FRANK W. SCHIFF, B.A. Columbia 1942. Economic impact of U.S. aid on selected Asian countries. *Columbia.*

CHARLES SIEGMAN, B.A. City College of New York 1957. Uses of export controls in international trade of primary producing economies. *Columbia.*

KENJI TAKEUCHI, B.A. Kwansee Gakauin (Japan) 1956. The special features of foreign investment in Japan, 1950-1959, and its effects upon her economic growth. *Duke.*

LEO TANSKY, B.S. Syracuse 1950; M.S. Columbia 1951. Comparative analysis of the impact of U.S. and U.S.S.R. economic aid to underdeveloped countries with special reference to India, Turkey and the United Arab Republic. *American.*

ORVEL L. TRAINER, B.A. Colorado 1950; M.A. 1955. The Scandinavian approach to the common market. *Colorado.*

DONALD A. WELLS, B.A. DePauw 1953; M.A. Virginia 1958. Servicing U.S. direct foreign investment. *Oregon.*

MARINA WHITMAN, B.A. Radcliffe 1956; M.A. Columbia 1959. Government participation in U.S. private foreign investment. *Columbia.*

JEFFREY G. WILLIAMSON, B.A. Wesleyan 1957; M.A. Stanford 1959. Long swings and United States balance of payments, 1800-1914. *Stanford.*

RICHARD B. WIRTHLIN, B.S. Utah 1956; M.A. 1957. The growth dynamics of linkages in the foreign sector. *California (Berkeley).*

Business Finance; Investment and Security Markets; Insurance

Degrees Conferred

WILLIAM H. L. ANDERSON, Ph.D. Harvard 1960. An econometric study of capital investment in the United States manufacturing industry, 1948-57.

HASKELL BENISHAY, Ph.D. Chicago 1960. Determinants of variability in earnings price ratios of corporate equity.

NAI-RUENN CHEN, Ph.D. Illinois 1960. Factors influencing investment expenditures.

ALAN B. COLEMAN, Ph.D. Stanford 1960. Financial management of foreign operations.

EDWIN B. COX, Ph.D. Pennsylvania 1960. Trends in the distribution of stock ownership.

LYNN E. DELLENBARGER, JR., Ph.D. Florida 1960. A study of relative common equity value in fifty mergers of listed industrial corporations, 1950-1957.

ROBERT S. FELTON, D.B.A. Indiana 1960. Selected problems faced by fire and casualty insurers under state and local taxation.

JOHN HALL, Ph.D. Pennsylvania 1960. The regulation of commercial health insurance for the individual.

CHARLES W. HOWE, Ph.D. Stanford 1959. A theoretical and empirical investigation of internal financing.

LAWRENCE D. JONES, JR., Ph.D. Harvard 1960. Portfolio objectives, external constraints, and the postwar investment behavior of life insurance companies.

MELVILLE PETERSON, Ph.D. Illinois 1960. A comparative study in debenture and mortgage bond financing.

NESTER R. ROOS, D.B.A. Indiana 1959. Government regulations of fire insurance.

STUART SCHWARTZCHILD, Ph.D. Pennsylvania 1960. The rights of creditors in life insurance policies.

DAVID A. SNELL, Ph.D. Texas 1960. Financial problems and the availability and adequacy of external financial resources for small firms.

FUAD H. TELLEW, Ph.D. Southern California 1959. Private foreign investment as a possible aid for the economic growth of Iraq.

DAN USHER, Ph.D. Chicago 1960. The debt-equity ratio.

SHENG W. WANG, Ph.D. Wisconsin 1959. Some fundamental problems of equipment investment planning.

WALTER WILLIAMS, Ph.D. Indiana 1960. Determining the actual cash value of commercial real property for insurance purposes.

Theses in Preparation

JOSEPH BELTH (earlier degrees not supplied). An analysis of participating ordinary life insurance issued by stock life insurance companies in the U.S. and Canada. *Pennsylvania.*

WILLARD T. CARLETON, B.A. Dartmouth 1956. Economic implications of the financial decisions of corporations. *Wisconsin.*

ROBERT CROWE (earlier degrees not supplied). Underwriting experience in automobile physical damage insurance. *Pennsylvania.*

CHARLES D'AMBROSIO, B.S. Loyola 1955; M.S. Illinois 1958. Structural changes in railroad financial plans: 1928-1958. *Illinois.*

WALTER S. FRANK, B.A. Harvard 1949; B.A. Oxford 1952. Portfolio behavior of the investment trusts. *Harvard.*

PAUL HAGGIPAVLOU, B.Sc. Ohio State 1957; M.A. 1958. Determinants of business investment—an empirical investigation in the steel industry. *Columbia.*

BOB E. HALL, B.S. Arkansas 1952; M.B.A. 1958. The variable annuity—the payout period. *Arkansas.*

VICTOR HALLMAN (earlier degrees not supplied). Comparison of compulsory automobile insurance with unsatisfied judgment funds. *Pennsylvania.*

ROBERT HUNGATE, B.A. Washington 1951. Inter-business financing. *California (Los Angeles).*

E. W. KIERANS, B.A. Loyola 1935. Financing corporate activity in Canada, 1936-58. *McGill.*

HERMAN I. LEIBLING, B.A. City (New York) 1935; M.A. American 1945. Financing of the postwar expansion of business: an industry analysis. *American.*

ROLAND T. MULLINS, B.A. Arkansas State 1956; M.B.A. Arkansas 1958. An investigation into the effects of inflation on the market for bonds. *Arkansas.*

JAMES T. MURPHY, B.Sc. Iowa 1954; M.A. 1957. An empirical study of the investment of three industries (1954-1958). *Iowa.*

J. RUSSELL NELSON, B.A. Pacific Union 1952; M.B.A. California (Los Angeles) 1957. The role of stock rights in corporate financial policy. *California (Los Angeles).*

MARSHALL PUCKETT (earlier degrees not supplied). Dividend policy; cost of capital; and stock price maximization. *Pennsylvania.*

JOHN C. RITCHIE, JR. (earlier degrees not supplied). Trends in internal financing 1915-1955, selected large manufacturing corporations. *Pennsylvania.*

ALEXANDER A. ROBICHEK, B.S. California 1956. Allocation of funds—mutual life insurance companies. *California (Berkeley).*

HOWARD J. SHERMAN, B.A. California (Los Angeles) 1950; J.D. Chicago 1953; M.A. Southern California 1957. Profit rates: relation to cyclical variations and corporate size, United States, 1931-1958. *California (Berkeley).*

PETER E. SLOANE, B.A. Yale 1947; M.A. 1958. Determinants of bond yield differentials, 1954-1959. *Yale.*

CECIL E. WALTON (earlier degrees not supplied). A study of some of the aspects of corporate refunding operations. *Arkansas.*

ROYALL WHITAKER (earlier degrees not supplied). Investigation of probability aspects of life rating and selection. *Pennsylvania.*

HOWARD WIDDOWSON (earlier degrees not supplied). Exchange of individual contracts in the area of life insurance and annuities. *Pennsylvania.*

Business Organization; Managerial Economics; Marketing; Accounting

Degrees Conferred

PETER C. BRIANT, Ph.D. Michigan 1960. The corporation income tax: its incidence and effects.

JOHN E. CHAMPION, Ph.D. Michigan 1960. Effectiveness of LIFO in the textile industry.

ELWYN K. DE VORE, D.B.A. Indiana 1960. A study of selected factors which may influence the movement of selected types of retailers away from the downtown shopping area of small cities.

HARALD EINSMANN, Ph.D. Florida 1960. The effects of the formation of the European economic community on managerial decisions of entrepreneurs in member countries.

PERSIS R. EMMETT, Ph.D. Stanford 1960. The development and location of shopping centers; and common location characteristics in selected areas in California.

WALTER D. GAINER, Ph.D. Mass. Inst. of Tech. 1959. Regional income leakages and the multiplier: Alberta, 1948-58.

JUDITH A. GROUSE, Ph.D. Harvard 1960. Executive compensation and how it is determined: the theory of mutual backpatting.

JOHN W. HENDERSON, JR., Ph.D. Wisconsin 1960. Import competition in manufacturing industries.

JIMMIE L. HESKETT, Ph.D. Stanford 1960. Industrial logistics: a movement system concept.

CHARLES H. HINDERSMAN, D.B.A. Indiana 1960. The changing balance of retail trade between downtown and outlying stores in metropolitan areas.

IRA HOROWITZ, Ph.D. Mass. Inst. of Tech. 1959. Economics of industrial research.

DONALD F. ISTVAN, D.B.A. Indiana 1960. The capital-expenditure decision-making process in forty-eight large corporations.

THOMAS F. KELLER, Ph.D. Michigan 1960. The interperiod allocation of corporate income tax.

TOSINOBU G. F. KITANO, Ph.D. Saint Louis 1960. A genetic study of German managerial policy of works community.

WALTER H. KRAMER, D.B.A. Indiana 1960. The role of the travel agent in the marketing of domestic air travel.

J. D. LANDES, Ph.D. North Carolina 1960. An analysis of classified advertising in newspapers.

CHI (DAVID) LUAN, Ph.D. Texas 1959. Statistical quality control in cotton marketing.

PAUL W. MACAVOY, Ph.D. Yale 1960. Price formation in natural gas fields.

GEORGE G. MILLER, Ph.D. Texas 1959. A critical analysis and appraisal of the theory and techniques of the management audit.

CHARLES V. MOORE, Ph.D. Ohio State 1959. An evaluation of farm accounting systems as aids to the management of commercial farms.

WALTER O'DONNELL, Ph.D. Columbia 1960. The value structure of corporate decisions.

TSVI OPHIR, Ph.D. Mass. Inst. of Tech. 1960. Intracompany pricing in the decentralized firm.

BEDROSE P. PASHIOIAN, Ph.D. Mass. Inst. of Tech. 1960. Automobile distribution: an economic analysis of the franchise system.

DONALD L. RICHARD, Ph.D. American 1959. The economic effects of the reducing charge methods of depreciation.

GEORGE SCHWARTZ, Ph.D. Pennsylvania 1960. Development of marketing theory (an evaluation of certain approaches being used in the development of marketing theory).

MAURICE SELDIN, D.B.A. Indiana 1960. An analysis of the impact of the firm on urban plant problems.

WILLIAM A. SHEPPARD, Ph.D. Mass. Inst. of Tech. 1960. Social aspects of operations research.

ALLAN T. STEELE, Ph.D. Texas 1960. A history of auditing in the United States, 1914-1957.

JAMES D. TAYLOR, Ph.D. Iowa 1960. A description, analysis, and partial explanation of changes in areal distribution of retail sales for selected groups in the central cities of ninety-five standard metropolitan areas from 1948 through 1954.

KENNETH P. UHL, Ph.D. Iowa 1960. Stockowners as customers for their corporations' products.

SHERWOOD G. WALTERS, Ph.D. New York 1960. Marketing in Brazil tested according to recent economic theory.

MARTIN R. WARSHAW, Ph.D. Michigan 1960. Manufacturer-wholesaler relations and their influence on pattern of distribution in selected consumer goods industries.

HILDA C. WASSON, D.B.A. Indiana 1959. Retail pricing policies and practices.

GERALD O. WENTWORTH, Ph.D. Stanford 1959. An evaluation of direct costing.

DELBERT E. WILLIAMSON, Ph.D. Stanford 1960. The concept of full disclosure in current accounting practice.

Theses in Preparation

DONALD K. ABE, B.S. Pennsylvania 1950; M.B.A. California 1955. Data for managerial control. *California (Berkeley).*

R. CLIFTON ANDERSEN, B.S. Indiana 1955; M.B.A. 1958. An analysis of the marketing wholesalers in the State of Indiana. *Indiana.*

ARTHUR F. BELOTE, B.S. Ohio State 1952; M.B.A. 1953. A study leading to the development of a more quantitative and systematic approach to hospital job evaluation. *Florida.*

ROBERT H. BOCK, B.S. Purdue 1954; M.S. 1955. An analysis of the long-range planning process in a select group of business firms. *Purdue.*

RALPH DAY, M.S. Georgia Institute of Technology 1955. Applications of linear programming to marketing. *North Carolina.*

JOHN H. DROGE, B.S. Kansas State 1953; M.S. North Carolina State 1959. Interregional competition in marketing Wisconsin commercial Irish potatoes. *Wisconsin.*

JOE R. FRITZEMEYER, B.B.A. Baylor 1956; M.B.A. Indiana 1957. Management and financial control of automobile dealer agencies. *Indiana.*

LEONARD J. GARRETT (earlier degrees not supplied). Information systems and managerial decision making and control. *Pennsylvania.*

JAC L. GOLDSTUCKER, B.B.A. Oklahoma 1940; M.B.A. Southern Methodist 1952. The dynamics of wholesale trading areas. *Minnesota.*

IVAN L. HALL (earlier degrees not supplied). A study of the problems of cost determination in the production of oil. *Arkansas.*

HERBERT G. HICKS, B.C.E. Georgia Inst. of Tech. 1954; M.B.A. Alabama 1958. The quantification of managerial decision problems. *Alabama.*

FRED J. HOFFER, B.S.A. Florida 1953; M.Ag. 1955. The cost of packing and distribution of Florida honey. *Florida.*

ARTHUR HOVERLAND, B.S. Miami 1951; M.S. Illinois 1954. A critical study of certain accounting principles and their possible incompatibility with management decision making. *Michigan.*

JOHN G. HUTCHINSON, B.S. Rhode Island 1951; M.S. 1953. Administration of production standards. *Michigan.*

MICHAL INGRAHAM, B.S. California (Los Angeles) 1952; M.B.A. 1953. The management of franchising organizations with particular reference to planning and control principals and practices. *California (Los Angeles).*

HAROLD KATZ, B.A. Brandeis 1956. The effects of electronic data processing innovations on diseconomies of management and optimum size of firms. *Columbia.*

ROBERT G. KOKAT, B.S. Pennsylvania State 1956; M.S. 1957. The impact of disarmament on the composition of industrial output. *Indiana.*

CRAIG LUNDBERG, B.A. Washington 1954; M.B.A. 1957. Decisioning in industrial organization. *Cornell.*

AVRUM W. W. MARKS (earlier degrees not supplied). An analysis of motivations for purchase and ownership characteristics of imported automobile (retailing at less than $2,000) owners residing in Phila., Pa. *Pennsylvania.*

ROBERT C. MEIER, B.S. Indiana 1952; M.A. Minnesota 1955. Administration of industrial operations research. *Minnesota.*

ROBERT J. MEYER, B.A. Massachusetts 1950; M.A. Connecticut 1951. An investigation of the economic consequences of the use of more capital intensive production. *Harvard.*

GERHARD G. MUELLER, B.S. California 1956; M.B.A. 1957. The accounting standards and practices of several foreign countries and their implications for financial analysis in the U.S. *California (Berkeley).*

WILLIAM NELSON (earlier degrees not supplied). The development of the concept of the firm. *Pittsburgh.*

FRANCESCO M. NICOSIA, Dott. Economia e Commercio Rome 1949. Decision making: toward a paradigm of consumer decision making. *California (Berkeley).*

JAMES OMPS (earlier degrees not supplied). A regional case study of economic methods and motivation of small business proprietors. *Pittsburgh.*

GEORGE B. SIMMONS, B.A. Louisville 1953; M.B.A. Indiana 1957. A theoretical framework for the evaluation of market potential in underdeveloped countries. *Indiana.*

JOHN F. STOLLSTEIMER, B.S. Michigan State 1953; M.S. 1957. The impact of the introduction of bulk handling techniques on the marketing of deciduous fruits. *California (Berkeley).*

LAWRENCE X. TARPEY, M.B.A. Indiana 1955. Some economic and administrative aspects of food chain advertising: a case study. *Cornell.*

RUSSELL A. TAUSSIG, B.S. California 1941; M.B.A. 1947. Use of accounting and economic costs in operations research models. *California (Berkeley).*

WILLIAM W. THOMPSON, B.S. Clemson 1953; M.B.A. Alabama 1958. A managerial history of a textile manufacturing company. *Alabama.*

HARVEY TSCHIRGI, M.B.A. Chicago 1949. An experimental study of interpersonal influence in executive decision making. *California (Los Angeles).*

C. EUGENE VINCENT, B.S. Indiana 1953; M.B.A. 1957. The application of control procedures in retail hardware stores. *Indiana.*

RICHARD J. WHITING, B.A. Washington 1947; M.B.A. Stanford 1949. An analysis of management functions as influenced by organized labor. *Southern California.*

FLOYD W. WILLIAMS, B.S. Virginia Polytechnic Inst. 1953; M.S. 1958. An evaluation of consumer preference and the effects of price variations and selected fruit characteristics on retail sales of Florida avocados. *Florida.*

Industrial Organization; Government and Business; Industry Studies

Degrees Conferred

ALLEN E. ABRAHAMS, Ph.D. New York 1960. An economic analysis of the American chemical nitrogen industry.

BEN ALVORD, Ph.D. Illinois 1960. A study of the financing of the U.S. trunk airlines, 1946-1955.

DANIEL BASILE, Ph.D. Cornell 1959. Development and critical analysis of the food program of the military forces of the United States.

ALBERT BUCKBERG, Ph.D. Michigan 1960. The interindustry structure among manufacturing industries in Michigan and Detroit, 1929-1955.

JAMES P. CAIRNS, Ph.D. Johns Hopkins 1960. Integration in the food retailing industry.

ALFRED B. CARLIP, Ph.D. Columbia 1960. The slide-fastener industry: a study of market structure innovations.

ELEANOR CRAIG, Ph.D. Duke 1959. Recent history of the North Carolina furniture manufacturing industry with special attention to locational factors.

HERSCHEL CUTLER, Ph.D. Syracuse 1960. Government transportation: the reduced rate controversy.

OTTO A. DAVIS, Ph.D. Virginia 1960. The economics of municipal zoning.

CHARLES R. DEAN, Ph.D. Columbia 1960. Industrial organization and monopoly behavior.

NORTON T. DODGE, Ph.D. Harvard 1960. Trends in labor productivity in the Soviet tractor industry.

FRED DZIADEK, Ph.D. Johns Hopkins 1960. The productivity of the U.S. Post Office: an intertemporal and cross-sectional study of post office labor productivity.

DANIEL O. FLETCHER, Ph.D. Michigan 1960. A study of package freight carriers on the Great Lakes.

MILTON S. GOLDBERG, D.B.A. Indiana 1960. Consent decrees in Sherman Act cases: an analysis of antitrust enforcement by negotiation.

F. J. HAYES, Ph.D. McGill 1960. The pulp and paper industry in Canada: an analysis of the restrictions on competition.

EUGENE C. HOLSHOUSER, Ph.D. Kentucky 1960. The effect of the Lexington, Kentucky, northern belt line on land values and land use: a case study.

WILLIAM R. HUGHES, Ph.D. Harvard 1960. The efficient organization of the privately owned electric utility industry in the United States.

RICHARD A. LA BARGE, Ph.D. Duke 1959. A study of United Fruit Company operations in Isthmian America, 1946-1956.

DONALD A. MARKWALDER, Ph.D. Northwestern 1960. The flour milling industry—an economic study of excess capacity.

CHARLES O. MEIBURG, Ph.D. Virginia 1960. The "free" public road and government highway policy decisions.

NORMAN A. MERCER, Ph.D. Harvard 1960. Growth and financing of the major firms in the electrical manufacturing industry.

EDWARD D. PETERSON, D.B.A. Indiana 1960. Selected issues arising from government regulation of the public utility industries.

CHARLES F. PHILLIPS, JR., Ph.D. Harvard 1960. Competition in the synthetic rubber industry.

STUART M. RICH, D.B.A. Indiana 1960. Electric home heating in the United States: its growth, industry, structure, equipment, promotion, marketing and outlook.

ROBERT A. ROBERTSON, Ph.D. Illinois 1960. An analysis of the motor vehicle industry as a factor in the economic life of Canada.

ROBERT M. ROESTI, Ph.D. Southern California 1960. Economic analysis of factors underlying pricing in the southern California tuna canning industry.

RASUL SALMAN, Ph.D. Columbia 1960. A distribution of gains from advancing productivity: in some concentrated and non-concentrated industries.

WILLIAM A. SANDRIDGE, Ph.D. Virginia 1960. The effects of fair trade on retail prices of electric housewares in Washington, Baltimore, and Richmond, 1952-1959.

WILLIAM D. SHIPMAN, Ph.D. Columbia 1960. An inquiry into the high cost of electricity in New England.

EARL A. SPILLER, Ph.D. Michigan 1960. Significance and influence of accounting data adjusted for price level changes on the problems and policies of public utilities.

MAX D. STEWART, Ph.D. Michigan State 1960. Some economic aspects of the Canadian wooden match industry and public policy.

FREDRIC STUART, Ph.D. Columbia 1960. The effects of television on the motion picture and radio industries.

HAROLD WEIN, Ph.D. Pittsburgh 1959. An integration of economic and legal approaches to growth in the steel industry.

PAUL WEINER, Ph.D. Clark 1960. Highway planning and pricing.

WESLEY J. YORDON, JR., Ph.D. Harvard 1960. Industrial concentration and prior flexibility in inflation: price response rates in fourteen industries, 1947-58.

ZENON S. ZANNETOS, Ph.D. Mass. Inst. of Tech. 1959. Theory of oil tankship rates.

Theses in Preparation

WILLIAM C. ADAMS, B.A. Arkansas State Teachers 1951; M.A. Arkansas 1953. The probable impact of inflation upon electric power company operations in Arkansas. *Arkansas.*

LEWIS N. AMIS, B.A. Peabody 1951; M.A. 1952. An economic history of gasoline: a case study in commodity economics. *Arkansas.*

MCDONALD P. BENJAMIN, D.I.C.T.A. Imperial College of Trop. Agric. (B.W.I.) 1945; M.A. California (Davis) 1956. California's fruit and vegetable canning industry, a study of the changing relationships of input in the industry. *California (Los Angeles).*

MICHAEL BORETSKY, Diplom-Volkswirt Erlangen 1949; Dr. rerum politicarum Free Univ. 1949. The cost and economies of scale in soviet-machine-tool building industry. *Columbia.*

JOHN CARLSON, B.S. Denison 1955. Investment decisions in the face of rapid technological progress: a case study of telephone exchanges. *Johns Hopkins.*

ARTHUR L. COBB, B.S. Florida 1950; M.A. 1952. The concept of the market in antitrust cases: a study in applied economics. *Indiana.*

BERT M. EVANS, B.S. Nebraska 1953; M.A. Texas 1954. Internal-external efficiencies and structural change in the perishable bakery products industry. *Harvard.*

IVAN HALL, B.S. Arkansas State Teachers 1956; M.B.A. Arkansas 1958. A study of the problems of cost determination in the production of oil. *Arkansas.*

R. B. HARSHBARGER, B.S. Manchester 1956; M.A. Indiana 1958. An analysis of TVA coal buying program. *Indiana.*

ROBERT S. HIMES, B.S. American 1951; M.B.A. 1955. The problem of stability in the machine tool industry. *American.*

DAVID HORLACHER (earlier degrees not supplied). Measurement of industrial capacity. *Pennsylvania.*

GEORGE R. HORTON, JR., B.S. Auburn 1952; M.S. 1953. An examination of the economic consequences of the consolidation of the southeastern railroads. *Virginia.*

EDWARD B. JAKUBAUSKAS, B.A. Connecticut 1952; M.A. 1954. Impact of technological change in the railroad industry—1947-1957. *Wisconsin.*

WALTER P. KLEIN, B.A. Wisconsin 1956; M.A. 1957. Some relationships between industrial concentration and market growth. *Wisconsin.*

EUGENE KOZIK (earlier degrees not supplied). Economic analysis utilized by the Supreme Court in its anti-trust decisions with special emphasis on the years 1946-59. *Pittsburgh.*

GERALD KRAFT, B.A. Wayne 1955; M.A. Harvard 1957. Methods of statistical costing in the transportation industry. *Harvard.*

R. F. LINDSAY (earlier degrees not supplied). A history of the natural gas industry in Canada. *Toronto.*

SUILIN LING, B.S. Michigan 1952; M.S. 1953. Economies of scale in the generation of electricity by steam. *Columbia.*

CHARLES MEYER, B.A. Illinois 1954; M.A. 1955. The cost function for the telephone industry: increasing or decreasing? *Johns Hopkins.*

RONALD E. MILLER, B.A. Harvard 1955; M.A. Washington 1957. The efficiency of the domestic air passenger transportation industry. *Princeton.*

NORMAN P. MONSON, B.S. Utah 1954; M.S. Columbia 1955. Incipient monopoly in industries competitively organized: lessons from U.S. vs. Brown shoe. *Columbia.*

THOMAS G. MOORE, B.A. Washington (St. Louis) 1957; M.A. Chicago 1959. Restrictions on entry and regulation of prices in the state of Illinois and the city of Chicago. *Chicago.*

RUSSELL C. PARKER, B.A. Washington State. Vertical integration in the retail food industry. *Wisconsin.*

WARREN PILLSBURY, B.A. New Hampshire 1953; M.S. Florida State 1958. The location of public highways: an economic analysis. *Virginia.*

RICHARD L. POLLOCK, B.A. Washington 1955; M.A. 1956. Profits in the defense industry. *Wisconsin.*

GEORGE D. QUIRIN, B.A. Alberta 1952; M.A. 1958. The regulation of field prices for natural gas under the Natural Gas Act. *Princeton.*

DONALD RICHMAN (earlier degrees not supplied). The small business administration. *Pennsylvania.*

PHILIP ROBBINS (earlier degrees not supplied). An analysis of economic problems of big business as reflected in and related to the writings of Ida M. Tarbell. *Pittsburgh.*

WILLIAM A. SANDRIDGE, B.A. Richmond 1943; M.A. Virginia 1956. The effects of fair trade on prices in Baltimore, Washington, and Richmond. *Virginia.*

WADE P. SEWELL, B.S. Illinois 1947; M.S. 1949. A stochastic production function of aircraft operation. *Chicago.*

ALBERT J. SIMONE, B.A. Tufts 1957. An economic analysis of public policy in the aluminum industry. *Massachusetts Inst. Technology.*

DONALD SOLAR, B.A. Wisconsin 1951. Federal Air Lines. *Columbia.*

GEORGE T. WEINER, B.A. Grinnell 1957. Piggyback and the Interstate Commerce Commission. *Massachusetts Inst. Technology.*

CLINTON H. WHITEHURST, B.S. Florida State 1957; M.A. 1958. The case for competition in American ocean shipping: a critique of government subsidy policy. *Virginia.*

Land Economics; Agricultural Economics; Economic Geography; Housing

Degrees Conferred

RENE BENALCAZAR, Ph.D. Wisconsin 1959. Toward a program for agricultural development in Ecuador.

MAX R. BLOOM, Ph.D. American 1959. Economic criteria and the use of land in subsidized urban redevelopment areas.

THEODORE BOREK, Ph.D. Pittsburgh 1959. Factors contributing to the growth and development of the arid southwest, with particular reference to the Phoenix metropolitan area.

HENRY BRAMER, Ph.D. Pittsburgh 1960. An analysis of the economic aspects of the water polution abatement case in the Ohio River valley.

WILLIAM H. CONKLE, Ph.D. Cornell 1960. The status of and trends in produce prepackaging in the northeast.

GILBERT DEMENTIS, Ph.D. Chicago 1960. Resource allocation in the lower Rio Grande River valley (1940-1956).

JOSEPH R. EWERS, D.B.A. Indiana 1959. Federal housing programs, 1960-70.

B. DELWORTH GARDNER, Ph.D. Chicago 1960. Misallocation of resources on federal range lands.

GEORGE R. HALL, Ph.D. Harvard 1960. The lumber industry and forest policy; a study in the economics of natural resources.

CURTIS C. HARRIS, JR., Ph.D. Harvard 1960. Direct production payments in agriculture.

JOHN R. HILDEBRAND, Ph.D. Chicago 1959. Geographical differentials in the earning power of input categories on Kansas farms.

PAUL H. HOEFNER, Ph.D. Minnesota 1960. An economic analysis of risk and uncertainty in dairy and hog production.

ANDREAS A. HOLMSEN, Ph.D. Cornell 1960. Variability in income and in factors affecting income on commercial dairy farms in the north country and central plain regions of New York state.

HARRISON HSIA, Ph.D. Wisconsin 1960. Economic decision-making in hog feeding—a new approach.

PHIMOL JITTEMANA, Ph.D. Wisconsin 1960. Agriculture in a developing economy—a mid-century appraisal of Thailand's agriculture.

PAUL R. JOHNSON, Ph.D. Chicago 1959. Land substitutes and changes in corn yields.

ARMAND L. LACASSE, Ph.D. Cornell 1959. Costs and efficiency in the operation of milk manufacturing plants in the New York-New Jersey milkshed.

ROBERT E. LAUBIS, Ph.D. Ohio State 1959. An analysis of the financial structure of agricultural cooperative business organizations in Ohio and suggestions for improvement.

JERRY M. LAW, Ph.D. Minnesota 1959. The development of a classification of market structures for agriculture.

GREGOR LAZARCIK, Ph.D. Columbia 1960. Production and productivity in Czechoslovak agriculture: 1934-38, 1946-59.

QUENTIN W. LINDSEY, Ph.D. Harvard 1960. The problem of periodic reorganization in agriculture.

ALLYN O. LOCKNER, Ph.D. Colorado 1960. A proposal for the taxation of molybdenum, uranium and vanadium mining in Colorado.

IRA S. LOWRY, Ph.D. California (Berkeley) 1959. Residential location in urban areas.

ALAN R. PLOTNICK, Ph.D. Pennsylvania 1960. Economic and commercial policy aspects of marketing western Canadian petroleum in Canada and the United States.

RONALD H. POLLOCK, Ph.D. Ohio State 1959. An analysis of changes in consumer milk purchases in two Ohio metropolitan areas.

STANLEY R. SCHULTZ, Ph.D. Ohio State 1960. An economic analysis of manufacturing milk production in Ohio.

RAM R. SINGH, Ph.D. Ohio State 1959. Reorganization of contiguous small farms for the maximum economic returns.

FOUAD TAWFIK, Ph.D. Texas 1959. Development and influence of the United States raw cotton policy on its economy.

GUSTAVO A. TEJADA, Ph.D. Ohio State 1959. Comparative returns to resources used on different types and classes of farms by major types of farming areas in Ohio and neighboring areas.

SISTER MARY E. THOMAS, B.V.M., Ph.D. Notre Dame 1960. A study of the causes and consequences of the economic status of migratory farm workers in Illinois, Indiana, Michigan, and Wisconsin from 1940-1958.

EDWIN R. WESTCOTT, Ph.D. Ohio State 1960. Optimum combinations of resources for dairy farms in west-central Ohio.

JOHN R. WILLIAMS, Ph.D. Columbia 1960. The premium price plan for copper, lead, and zinc.

Theses in Preparation

MARTIN E. ABEL, B.S. Cornell 1956. Programs for expanding the demand for farm food products in the United States. *Minnesota.*

DAVID J. ALLEE, B.S. Cornell 1953; M.S. 1954. Institutional arrangements for irrigation in New York. *Cornell.*

DAVID L. ARMSTRONG, B.S. Ohio State 1957; M.S. 1958. An analysis and comparison of the economics of forage management systems. *Ohio State.*

ABDEL-MAWLA M. BASHEER, B.S. Alexandria 1951; M.S. Cornell 1958. The land settlement approach to economic development—a comparative study. *Wisconsin.*

H. H. F. BINHAMMER, B.A. Western Ontario 1948; M.A. Queen's 1957. Study of the Canadian housing sector. *McGill.*

OSWALD P. BLAICH, B.S. Manitoba 1946; M.S. Minnesota 1955. The theory of vertical integration with an application to the feed processor-hog producer-meat packer complex. *Minnesota.*

CARROLL G. BRUNTHAVER, B.S. Ohio State 1954; M.S. 1958. An economic analysis of live and carcass lamb marketing structure in Ohio. *Ohio State.*

GIOVANNI CODA-NUNZIANTE, Dr. in Agri. Sc. Naples 1952. International trade in lemons and lemon products. *California (Berkeley).*

DAVID G. DAVIES. Agriculture response to urban industrial development. *North Carolina.*

MARCEL F. E. DE BACKER, B.A. Royal Lycée (Antwerp) 1942. An economic appraisal of forest-policy structures in the mountainous savanna regions of eastern Central Africa. *California (Berkeley).*

VINCENT F. DUNFEY, B.A. Boston College 1937; M.A. 1947. An analysis of the demand for haddock. *Boston College.*

MARY P. DURBIN, B.S. Reading (England) 1955; M.S. California (Davis) 1957. The economics of water importation, with special reference to problems of drainage. *California (Berkeley).*

SALOMON R. ECKSTEIN, Lic. en Economie, Mexican 1957. Collective farming in Mexico. *Harvard.*

CARL K. EICHER, B.S. Michigan State 1952; M.S. 1956. Economic development policy and planning for American Indian rehabilitation programs. *Harvard.*

AUSTIN B. EZZELL, B.S. Alabama Polytechnic Inst. 1948; M.S. Michigan State 1956. Some economic impacts of selected types of legislation on food distribution. *Ohio State.*

HOMER FAVOR (earlier degrees not supplied). The effects of racial changes in occupancy patterns upon property values in Baltimore. *Pittsburgh.*

LEHMAN B. FLETCHER, B.S. Florida 1954. Economic organization and growth in the Los Angeles milkshed. *California (Berkeley).*

HELMUT J. FRANK, B.S. Columbia 1948; M.A. 1950. The pricing of Middle East oil. *Columbia.*

MARTIN J. GERRA, B.S. Georgetown 1949; M.A. Catholic 1954. Microeconomic forecasting—a case study on the price of eggs. *Johns Hopkins.*

PETER G. HELMBERGER, B.S. Minnesota 1955; M.S. 1957. The economic impact of bargaining cooperatives in the production and distribution of fruits and vegetables. *California (Berkeley).*

STEPHEN J. HIEMSTRA, B.S. Iowa (Ames) 1953; M.S. 1957. Structural changes in California food retailing. *California (Berkeley).*

JAMES G. HILTON, B.S. North Carolina State 1954. An application of inventory theory to farm equipment repair parts. *Iowa (Ames).*

IRVING E. JOHNSON, D.I.C.T.A. Imperial College of Trop. Agri. (B.W.I.) 1942; M.S. Cornell 1950. Land tenure in Jamaica. *Cornell.*

EDITH M. JONES, B.Sc. London 1940; M.S. California 1959. An economic analysis of agricultural market controls under government regulation—a comparative international survey. *California (Berkeley).*

JOHN F. KAIN, B.A. Bowling Green State 1957. A theory of residential location. *California (Berkeley).*

ROBERT KARG (earlier degrees not supplied). Theory and practice of the firm applied to the crude petroleum industry. *Pittsburgh.*

DONALD G. LARSON, B.S. Iowa (Ames) 1956. Analysis of enumerator variance in agricultural censuses. *California (Berkeley).*

TONG H. LEE, B.S. Chosun Christian 1955; B.A. North Eastern Missouri 1956; M.A. Wisconsin 1958. A decision unit model of housing expenditure in the United States. *Wisconsin.*

CHARLES Y. LIU, B.A. National Taiwan 1954; M.S. Montana State 1957. Short-term distribution of meat production by regions. *Iowa (Ames).*

WILLIAM E. MARTIN, B.S. California (Davis) 1954. An interindustry analysis of California emphasizing agriculture. *California (Berkeley).*

LUIS A. MEJIA-MATTEI, B.A. Puerto Rico 1949; M.S. Wisconsin 1958. Integration in agricultural marketing with special emphasis on cooperative marketing. *Wisconsin.*

ELMER L. MENZIE, B.S. British Columbia 1952; M.S. 1955. Section 22 of the Agricultural Adjustment Act of 1933: a study in public policy formation and administration. *California (Berkeley).*

GLENN H. MILLER, JR., B.A. Kansas 1952; M.A. 1954. American agriculture in the 19th century. *Harvard.*

DON B. MILLIKEN, B.S. Purdue 1952; M.S. 1953. Enhancing the contribution of accounting information to the effective management of fluid milk plans. *Purdue.*

WILLIAM C. MOTES, B.S. Kansas State 1954; M.S. 1958. Effects of transportation costs on the location of the meat packing industry. *Iowa (Ames).*

RALPH E. NELSON, B.S. Minnesota 1949; M.S. 1952. The nature of competition among dairy processing plants in South Dakota. *Minnesota.*

HUGH O. NOURSE, B.A. Washington (St. Louis) 1955; M.A. Chicago 1959. The effect of public housing on property values in St. Louis. *Chicago.*

JOSE A. OLIVIERI-RODRIGUEZ, B.S. Puerto Rico 1950; M.S. Wisconsin 1955. An evaluation and analysis of research in agricultural economics in the north central region of the U.S. *Wisconsin.*

MANCUR L. OLSON, B.C. North Dakota Agri. 1954; B.A. Oxford 1956. The American Farm Bureau Federation: an illustration of a concept of collective action. *Harvard.*

SURYAMANI PATHAK, B.Sc. State College of Agri. (India) 1951; M.Sc. 1953. Relationship between various input-output factors in agriculture, western U. P. India. *Cornell.*

RUTH RASCH, B.A. Bryn Mawr 1957. Effects of monetary and public lending policies on residential construction. *Johns Hopkins.*

ROBERT E. RIECK, B.S. Wisconsin 1950; M.S. 1957. An empirical measure of decision-making ability among Wisconsin farmers. *Wisconsin.*

R. STEPHEN RODD, B.A. British Columbia 1950; M.Sc. London School of Econ. The economic structure of metropolitan agglomeration. *Massachusetts Inst. Technology.*

ORLANDO J. SACAY, B.S. Philippines 1954; M.S. Cornell 1956. An analysis of the crop loan operation of the agricultural credit and cooperative financing administration. *Cornell.*

WILLIAM N. SCHALLER, B.A. Princeton 1951. An aggregative programming analysis of regional production response. *California (Berkeley).*

E. C. SIEVWRIGHT, B.Sc. London 1950; M.A. McGill. Impact of the oil industry on the economy of Alberta. *McGill.*

ABDUL H. H. TAHER, B.Comm. Ain-Shams 1955. Allocation of costs between crude oil and natural gas. *California (Berkeley).*

RICHARD W. TRESTRAIL, B.S. Minnesota 1950. Valuation of forests in the U.S. for taxation purposes. *Washington.*

HENRY A. WADSWORTH, JR., B.S. Cornell 1956; M.S. 1959. Economics of large dairy farms in New York State. *Cornell.*

JAMES C. WILLIAMSON, JR., B.S. North Carolina State 1943; M.S. 1950. Grade price relations in flue-cured tobacco. *Chicago.*

ELMER E. ZANK, B.S. Wisconsin 1954; M.S. 1956. Structure of the grain marketing industry. *Wisconsin.*

PINHAS ZUSMAN, M.S. Hebrew (Israel) 1956. Econometric analysis of California early potato market. *California (Berkeley).*

Labor Economics

Degrees Conferred

ABRAHAM H. BELITSKY, Ph.D. Harvard 1960. Hiring problems in the building trades with special reference to the Boston area.

ELLIOT J. BERG, Ph.D. Harvard 1960. Recruitment of a labor force in sub-Saharan Africa.

EDWIN W. BISHOP, Ph.D. Wisconsin 1959. The Guatemalan labor movement, 1944-1959.

DONALD T. BUTLER, Ph.D. Wisconsin 1959. Trade union organizing in Iowa, 1945 to 1956.

JOHN H. G. CRISPO, Ph.D. Mass. Inst. of Tech. 1960. Collective bargaining in the public service: a study of union-management relations in Ontario Hydro and TVA.

J. KENNETH DAVIES, Ph.D. Southern California 1959. A study of the labor philosophy developed with the Church of Jesus Christ of Latter Day Saints.

WILLIAM S. DEVINO, Ph.D. Michigan State 1960. Unemployment compensation exhaustees during a recession.

ABDUL S. N. EL-SHAHIN, Ph.D. Southern California 1960. Minimizing Iraq's disguised unemployment through industrial development.

EDMOND L. ESCOLAS, Ph.D. Clark 1960. A study of Wyoming's workmen's compensation system, 1915-56.

ROBERT EVANS, JR., Ph.D. Chicago 1959. The economics of American Negro slavery, 1830-1860.

THOMAS A. FINEGAN, Ph.D. Chicago 1960. Hours of work in the United States—a cross sectional analysis.

HOWARD M. GITELMAN, Ph.D. Wisconsin 1960. Attempts to unify the American labor movement, 1865-1900.

ARANKA KOVACS, Ph.D. Bryn Mawr 1960. The theory of wages in the Ricardian environment.

ARTHUR M. KRUGER, Ph.D. Mass. Inst. of Tech 1959. Labour organization and collective bargaining in the Canadian basic steel industry.

BEVARS D. MABRY, Ph.D. Tulane 1959. An analysis of state labor laws regulating trade union activities.

GARTH L. MANGUM, Ph.D. Harvard 1960. Wildcat strikes and union pressure tactics in American industry: a case and general study.

HAZEL MCCALLEY, Ph.D. Pennsylvania 1959. Collective bargaining among professional workers employed by selected nonprofit organizations.

CHARLES R. MILTON, Ph.D. North Carolina 1960. The development of philosophies of personnel administration.

MARTIN P. OETTINGER, Ph.D. Harvard 1960. The industrial relations system in the Netherlands with special emphasis on wage policy since the end of World War II.

THOMAS J. REYNOLDS, Ph.D. Columbia 1960. Factory employment in New Jersey 1899-1956: growth and structural change.

SUMNER M. ROSEN, Ph.D. Mass. Inst. of Tech. 1959. Labor in Turkey's economic development.

KARL RUPPENTHAL, Ph.D. Stanford 1959. Revolution in the Air Line Pilots Association.

HENRY SANBORN, Ph.D. Chicago 1960. Income differences between men and women in the United States.

GEORGE S. SAUNDERS, Ph.D. Wisconsin 1959. The movement of union and nonunion wage rates in the Ontario iron and steel products industries, 1946-1954.

KENNETH T. STRAND, Ph.D. Wisconsin 1959. Jurisdictional disputes among the building trades unions.

S. HERBERT UNTERBERGER, Ph.D. Pennsylvania 1960. The evolution of wage incentive systems.

WARREN C. WATERHOUSE, Ph.D. Northwestern 1960. Causal factors underlying the relationship between wage rates and job content in the industrial firm.

Theses in Preparation

PEL-YANG CHANG, Soochow 1940; LL.M. New York (Law) 1953. Picketing. *New York.*

ALBERT CLARK (earlier degrees not supplied). An analysis of the development and operations of the employee retirement system operated by the city of Philadelphia. *Pennsylvania.*

ROSANNE COLE, B.A. Miami 1955. Discrimination as an explanation of unemployment differentials. *Columbia.*

MARY O. CONLON, B.A. Queen's 1954; M.A. 1956. Secular and cyclical movements of skilled/unskilled wages in Canada. *Columbia.*

ANDREW J. COOPER III, B.S. Georgia Inst. Technology 1953; M.S. 1954; M.A. Princeton 1959. Air Line Pilots Association. *Princeton.*

N. F. DAVIS, B.S. Lincoln 1949; M.B.A. Washington (St. Louis) 1952. Trade unions' practices and the Negro worker—the establishment of AFL-CIO antidiscrimination policy. *Indiana.*

JULIA DE VINCENTI, M.S. New York 1940. Developmental history of organized labor in Puerto Rico. *Cornell.*

DORIS DRURY, B.A. Western Ontario 1949; M.A. Indiana 1950. The concept of bargaining in the theory of unions. *Indiana.*

J. D. DUNN, B.A. Texas 1950; M.B.A. 1955. White-collar unionization in the deep South. *Alabama.*

ELDON J. DVORAK, B.S. South Dakota State 1953. Effects of changes in the labor-force structure upon labor-management relations. *Washington.*

G. E. EATON, B.A. McGill 1957. Developments of trade unions in Jamaica. *McGill.*

JOHN FERGUSON, B.A. Stanford 1933; M.B.A. 1935. Job satisfaction and productivity in a university faculty. *Cornell.*

GERALD E. FITZGERALD, B.A. Brooklyn 1945; M.P.A. City (New York) 1956. Current practices in wage administration and position classification administration in selected American jurisdictions. *New York.*

HARRY J. GILMAN, M.A. Chicago 1958. The role of discrimination in unemployment. *Chicago.*

CHARLES P. HALL (earlier degrees not supplied). Analysis of special compensation paid to salaried employees of life insurance companies. *Pennsylvania.*

ROBERT I. HISLOP, LL.B. St. Lawrence 1938; LL.M. 1939. The international labor organization. *Colorado.*

MARY D. HOUSKA, B.S. Simmons 1954. The substitution of engineers for men with less technical education. *Massachusetts Inst. Technology.*

MAXIMILIAN B. JONES, M.S. North Carolina 1955. Wage structure theory and its implications for job evaluation. *North Carolina.*

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JAY B. KENNEDY, B.A. Indiana 1954; M.A. 1957. Protective labor legislation in Indiana. *Indiana.*

JOSEPH J. MELONE (earlier degrees not suppl'ed). Impact of collective bargaining on private pension plans. *Pennsylvania.*

MURRAY B. NESBITT, B.A. New York 1948; LL.B. 1950. Administration and UE process in the National Labor Relations Board. *New York.*

JOHN C. O'BRIEN, B.Comm. London 1952; M.A. Notre Dame 1959. An analysis of standards in the administration of welfare and pension plans. *Notre Dame.*

LEONARD A. RAPPING, B.A. California (Los Angeles) 1956; M.A. Chicago 1959. The impact of unionism and government subsidies on the relative wages of seamen. *Chicago.*

RAY C. ROBERTS, M.S. North Carolina 1957. Collective bargaining in the trucking industry. *North Carolina.*

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KARL SAUBER, B.S. Kent State 1956. An economic analysis of the 1959 steel strike. *Illinois.*

WARREN P. SAUNDERS, JR., B.A. Penn State 1956; M.A. Illinois 1957. The political dimension of labor-management relations in Massachusetts: its economic impact. *Massachusetts Inst. Technology.*

HAROLD SIMMONS, B.A. Western Michigan 1946; M.A. Michigan State 1953. The UAW-AFL: a study of an AFL enclave in the auto industry, 1939-1960. *Michigan State.*

NAM WON SUH, B.S. Seoul 1954; M.S. San Francisco State 1958. An analysis of organized labor movements: the concept of limited-purpose organization. *California (Berkeley).*

JOHN P. SUSKO, B.S. Pittsburgh 1932; M.A. 1933. An analysis of the impact on prices of the escalator clause in collective bargaining agreements. *Notre Dame.*

RITA F. TAUBENFELD, B.A. New York 1946. Labor readaptation and industry rationalization: a study in factor mobility. *California (Berkeley).*

WILLIAM J. WASMUTH, B.S. Jefferson 1944; M.B.A. Washington (St. Louis) 1955. The administrative feasibility of flexible retirement programs in selected companies. *Indiana.*

C. GLYN WILLIAMS, B.A. University (Wales) 1956; M.A. Manchester 1958. A comparative study of labor practices in railroading in the United States and Great Britain. *Virginia.*

FRANK WIRIG (earlier degrees not supplied). Employee benefit plans of American religious organizations. *Pennsylvania.*

HAROLD WOOL, B.S. Brooklyn 1936; M.S. New School 1939. Skilled manpower needs and resources of the armed services—trends and implications. *American.*

DONALD C. WRIGHT, B.S. Iowa 1940; M.A. 1949. The unionization of the registered nurse. *Iowa.*

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MARCUS ALEXIS, Ph.D. Minnesota 1959. Racial differences in consumption and automobile ownership.

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HUGH W. FOLK, Ph.D. Duke 1960. The income and employment of the aged.

JACK MINKOFF, Ph.D. Columbia 1960. The Soviet social insurance system since 1921.

ELLA J. POLINSKY, Ph.D. American 1959. Some implications of the employment patterns of women under social security.

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DAVID H. CLARK, B.A. Oklahoma 1954; M.S. Wisconsin 1960. The national health bill: its history and future. *Wisconsin.*

JOHN A. DELEHANTY, B.A. Miami 1949; M.A. 1956. Financing unemployment benefits in Indiana: as assesement of past experience and outlook for the immediate future. *Indiana.*

PAUL G. DEMENY, M.A. Princeton 1959. Investment allocation and population growth. *Princeton.*

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JOSEPH GARTNER, A.A.S. L.I. Agri. & Tech. Inst. 1951; B.S. Connecticut 1954; M.S. New Hampshire 1956. An application of economic models to consumer marketing education. *Iowa (Ames).*

LOIS S. GRAY, B.A. Park 1943; M.A. Buffalo. The net gains from migration—Puerto Rico to New York. *Columbia.*

KHONDKAR T. HOSAIN, B.A. Dacca (Pakistan) 1951; M.A. 1952. The role of demographic variables in economic development as considered in the literature of the United Nations. *Duke.*

KAROL J. KROTKI, B.A. Cambridge 1948; M.A. 1952. The demography of Sudan. *Princeton.*

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ADOLPH MARK, B.A. DePaul 1950; M.A. Michigan 1954. Some economic and social relationships between changing household technology and family labor force behavior—1920-60. *Illinois.*

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S. E. ROTENBERG (earlier degrees not supplied). Some aspects of the economics of retirement. *Toronto.*

ELEANOR M. SNYDER, B.A. Connecticut College for Women 1936; M.A. Columbia 1938. Freedom from want. *Columbia.*

PAUL T. THERKILDSEN, B.S. Bradley 1951; M.S. Colorado 1957. Public welfare; a positive micro institutional and normative macro institutional approach. *Colorado.*

YASUKICHI YASUBA, B.A. Tokyo 1953; M.A. 1956. Economics of birth rates in the U.S., 1800-1960. *Johns Hopkins.*

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The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are Feburary 1, May 1, August 1, and November 1.

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